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PERIODS OF ARRESTED PROGRESS IN THE ACQUISITION OF SKILL.

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I. REVIEW OF THE PROBLEM.

A STUDY of the literature dealing with the occurrence of plateaus in the process of learning shows a fairly clear-cut distinction between those workers who regard the plateau as due to factors inherent in the learning process, and those who suppose it to be the result of a temporary failure in attention and effort on the part of the subject. The distinction is of practical as well as theoretical importance, because in the one case the plateau would probably be inevitable and essential, while in the other case it might be eliminable by means of a suitable adjustment of incentives.

Those workers who have taken the first point of view have all assumed that the occurrence of plateaus was intimately connected with the fact that the tasks in which they appeared consisted of dissociable parts. Thus Bryan and Harter (1897, 1899) distinguished 'lower-order' and 'higher-order' habits, which in their experiments on the reception of telegraphic messages roughly corresponded to the ability to receive words, and the ability to receive whole phrases and sentences as units. A plateau occurred because the two sets of habits were not improved simultaneously from the start, but there was a period of lag between the perfection of the lower habits and the inception of the higher habits during which no progress was shown. At this time the lower habits were becoming automatic, thus freeing

the attention for the higher habits. The standpoint of Swift (1903–1910), in his ball-tossing and other experiments, was essentially the same.

The alternative theory, which ascribes the plateaus to failure of attention and effort, was originally put forward by Johnson (1898), and was elaborated at great length by Book (1908, 1925). According to Johnson, "if the subject could be induced to sustain the same effort day by day, there would be no plateaus." Book concluded from introspections and tests of pulse-rate that less effort was actually put into the work at all those stages of practice where little or no improvement was made. Later, however, he modified this position by stating that the plateaus tended to occur at critical stages in the learning process—at certain definite levels of advancement, and apparently at points where new and increased difficulties were met with.

Bryan and Harter and Swift, in the course of their papers, made statements which to some extent approximated their views to that of Book. Bryan and Harter mentioned that "men whose receiving curve has been on a level for years frequently rise to a far higher rate when forced to do so, in order to secure and hold a position requiring the higher skill." Swift mentioned a lapse of attention as accompanying plateau periods. More recently, Thurstone (1919), in typewriting tests, found plateaus in only 2 out of 83 subjects, and considered that in both cases the explanation probably was that these subjects were offered commercial positions after attaining a certain efficiency, and consequently made little effort to improve beyond this point.

Thus, the difference between the two schools is far less fundamental than it seems at first sight, since on the one hand Book apparently admitted that the plateaus tend to occur at certain definite stages of the learning process, while on the other hand Swift and Bryan and Harter apparently admitted that they may be accompanied by diminished attention and terminated by a renewed effort. The two views are not mutually exclusive.

Batson, in 1916, carried out a series of experiments to test the supposition that plateaus, if they occurred, were probably connected with the degree of complexity of the task as Bryan and Harter had suggested. He confirmed Swift's result that no plateaus occurred in the relatively simple task of ball-tossing. This was still the case even when the subject was directed to concentrate his attention on one or

other phase of the game. Batson then devised a more complicated task consisting of four very distinct stages, and from his results concluded that plateaus occurred with those subjects who gave their attention to the separate factors of the task, but not with those subjects who tried to make progress in all the parts simultaneously. The score in which plateaus occurred was the score for successful performance of the whole task. Unfortunately the test was such that successful performance of any one part presupposed success in the preceding parts. Hence, if progress was only being made in the earlier parts, the score as a whole would necessarily show no improvement. The scores and introspections did not conclusively eliminate the possibility that the plateaus were incidental in nature rather than due to a concentration of attention on one part of the task.

Two negative results may, at any rate, be regarded as established by the work done during the last thirty years; first that plateaus do not as a rule occur in the learning of a relatively simple process (Bryan and Harter, 1897, 1899); secondly that even in the learning of a relatively complex process they do not necessarily occur (Batson, 1916).

The experiments which follow were planned in order to gain further evidence as to the existence and causes of plateaus in tasks of three types. Task A was a rather complicated ball-game, 'Ringball,' which was so arranged that fresh elements could be introduced at will, in order to study the process of co-ordinating them with the older ones; or alternatively it could be played at its highest degree of complexity from the outset; or, lastly, the elements could at first be practised separately, one after the other, and later incorporated into a combined movement. Task B was a parlour game known as 'Guidit,' which differed from 'Ringball' in requiring only eye and hand movements; Task C was a simplified type of 'Shorthand,' which differed from the other tasks in involving a larger proportion of mental activities.

II. Task A: Experimental method.

In bare essentials this game consisted of throwing a rubber ball into a target on the wall, or in causing it to bounce into a target on the floor. These two items could be practised separately or together, and with the right hand only or alternatively with the right and left hands. Two forms of the game were employed.

Ringball, Type 1.

The first section of this game involved only a right-handed throw. The subject aimed at tossing the ball into the right-hand target on the wall, and caught it again after it had bounced once on the floor. To begin with he tried to throw in time to a metronome set at 60 beats per minute, the separate beats marking throw, bounce and catch respectively, so that actually he threw 20 times per minute if the rhythm was perfect. The rate of the metronome was accelerated either when the subject asked for this to be done, or when the speed attained seemed to warrant such a change. The subject was allowed to pass on to the next section when he had scored an average of 16 out of 20, three days in succession.

In the second section this learned movement was combined with others which had not previously been practised. The subject was now required to throw alternately with the right and left hand, and also to endeavour to make the right-hand balls bounce inside the target on the floor. In the third section bouncing into the left-hand target was also introduced.

Each practice consisted of 5 sets, and each set consisted of 20 throws when only one hand was used, and of 40 throws when both hands were used. An interval of three minutes was allowed between each set. One point was allowed for each shot that entered the target on the wall, and 1 point for each time the ball was caught on the return. In the first section of the game these were the only items scored.

In the second section 1 point was allowed in addition for each ball bouncing inside the floor target. Further, when the left hand was introduced 1 point was allowed for each ball thrown into the left-hand target, and 1 point for each left-hand catch.

In the third section 1 point was also scored for each shot bouncing inside the left-hand floor target. Seven subjects practised this form of the game, but two of them not for long enough to learn the first section.

Ringball, Type 2.

A difficulty encountered with the original form of Ringball was that the daily fluctuations were large relatively to the total score. Most subjects started at a score of about 8; while the upper limit was 15 or 16. The daily fluctuations were sometimes as much as 3 or 4 points, so that they masked the form of the curve. The second form of the game was devised in order to provide a larger score, with relatively smaller fluctuations.

The single hit-or-miss target was replaced by a target consisting of three concentric circles, the annular areas between which were painted in different colours.

Ten points were now allowed for each bull on the targets, 6 for the ring immediately outside the bull, and 3 for the outer ring; intermediate scores of 8, 4 and 1 respectively were allowed for shots on the bounding circles. Each practice consisted of 5 sets, each of 20 shots with the right hand and 20 shots with the left hand, the two hands being used alternately.

Of the four subjects who practised this second form of the game, two (B. S. and M. V.) were required to practise the complete movement, both hands throwing and bouncing from the outset. The other two (L. S. and M. M.) began by practising the right and left hands on the same occasion, but in successive sets instead of alternately. After practice 6 they threw alternately with right and left hands, and after 12 practices they attempted also to make the balls bounce inside the floor targets.

In both types of the task it was found that the errors of any one subject usually had a distinct bias to one side or other of the target. In order to obtain a quantitative measure of this bias, the ratio $(R - L)/(R + L)$ was calculated for each set of throws. R represents the number of errors to the right of the target, L the number to the left of the target. The ratio is equal to zero when the errors are equally distributed on either side of the target, and has maximum numerical values of $+ 1$ or $- 1$ when the errors are all to the right or all to the left. A similar expression, namely $(U - D)/(U + D)$, where U is the number of errors made above the target, and D the number made below the target, serves to define the vertical bias. A decrease in the horizontal or vertical bias did not, of course, necessarily indicate that the errors were less serious, but it did indicate that they were more uniformly distributed about the target, and in certain cases this denoted an improvement in achievement which was not given credit for in the score. Considerations of space prevent the reproduction of any of these figures.

III. Periods of arrested progress in Task A.

The expression 'period of arrested progress,' was used to avoid any preconceived ideas which might be associated with the term 'plateau.' It was arbitrarily defined to include any period during which the score did not improve for six or more successive practices. A few

shorter periods were admitted where the arrest was marked and an explanation in terms of day-to-day fluctuations very unlikely.

The nine subjects who learned Task A all showed periods of arrested progress in their total scores, as will be seen from the following table, in which such periods are denoted by the numbers of the practices over which they extended.

Table I. *Periods of arrested progress.*

Ringball, Type 1.

Subject K. P. Practices	Subject D. S. Practices	Subject N. D. Practices	Subject F. B. Practices	Subject R. P. Practices
13–18	8–17	13–29	23–26	14–19
22–28	23–52	(31–49)*	32–35	33–42
29–38	72–79	—	36–44	—
49–53	87–99	—	—	—
55–59	(See Fig. 1)	—	—	—

Ringball, Type 2.

Subject B. S. Practices	Subject M. V. Practices	Subject M. M. Practices	Subject L. S. Practices
22–42	7–17	11–20	6–9
—	(22–40)*	22–26	11–16
—	58–62	30–32	20–27
—	68–89	—	40–44
—	(See Fig. 2)	—	56–64

* Very slight progress was made during this period.

Of the 29 periods thus enumerated, only 12 extended over 12 or more practices, and only 2 over 20 or more practices. The most extreme case was that of Subject D. S., who made no improvement for 30 practices (23–52 inclusive). At the other extremity the list contains one or two periods which might conceivably be merely the result of a previous accidental high score or some such chance effect.

In attempting to account for these periods, evidence was available from two sources, first from an analysis of the curves themselves, and secondly from the introspections. The latter have been used cautiously, and where there was any conflict of evidence the main weight has been placed on the purely objective data given by the curves. In some cases this alone was sufficient to determine the probable cause of the arrest of progress. Only a selection of the learning curves and scores obtained can be given in this paper. These will be found in Figs. 1 and 2 and in Table II.

Fifteen of the periods of arrested progress can be eliminated as being more or less obviously due to accidental factors. The remaining 14 were apparently due to causes inherent in the learning process, for the particular subject.

Table II. *Ringball, Type* 2. *Total score (thrice-weekly practices).*

No. of practice	Subjects				No. of practice	Subjects		
	B. S.	M. V.	L. S.	M. M.		B. S.	M. V.	L. S.
1	57·1	31·4	45·8	51·3	46	135·4	61·9	98·5
2	82·8	41·2	60·6	68·2	47	131·3	74·7	105·9
3	83·4	46·2	74·2	66·3	48	135·2	77·6	106·3
4	76·8	43·2	66·2	68·1	49	130·3	66·1	112·4
5	82·7	44·6	76·8	69·3	50	137·4	71·9	104·7
6	93·1	56·2	[74·4	72·6	51	137·7	85·6	110·6
7	86·8	[50·4	61·3	80·1	52	134·9	75·4	104·9
8	99·9	52·1	59·6	72·1	53	137·9	84·2	87·0
9	96·0	46·5	67·6]	81·4	54	—	79·2	113·6
10	83·0	42·0	78·5	86·2	55	—	78·8	119·3
11	87·2	48·8	[76·4	[80·7	56	—	87·2	[99·2
12	96·8	53·1	70·3	81·7	57	—	94·1	106·9
13	95·1	51·8	73·9	77·2	58	—	[89·5	105·7
14	93·0	56·6	74·7	83·4	59	—	86·6	*109·9*
15	107·6	48·1	*56·9*	85·4	60	—	78·8	113·4
16	106·4	48·8	62·8]	*77·0*	61	—	93·6	106·4
17	106·7	48·2]	76·7	82·6	62	—	89·9]	105·3
18	116·8	60·2	72·8	82·4	63	—	93·0	95·5
19	117·0	54·8	77·2	77·2	64	—	*76·8*	88·2]
20	122·5	65·4	[64·9	80·8]	65	—	92·2	—
21	122·1	68·2	63·4	88·6	66	—	86·0	—
22	[*99·4*	[*43·6*	65·8	[85·0	67	—	96·7	—
23	113·6	49·2	72·4	84·5	68	—	[80·8	—
24	113·2	45·8	73·2	63·3	69	—	83·7	—
25	120·6	53·4	73·6	85·6	70	—	88·7	—
26	107·6	41·8	76·4	85·2]	71	—	81·2	—
27	107·0	55·6	69·2]	90·4	72	—	80·9	—
28	109·8	52·4	79·0	84·0	73	—	83·5	—
29	123·4	44·2	80·8	94·7	74	—	92·3	—
30	115·2	61·1	94·4	[90·8	75	—	84·3	—
31	121·2	47·4	82·8	88·0	76	—	85·3	—
32	119·8	48·6	94·3	82·4]	77	—	87·4	—
33	119·4	46·6	84·8	99·9	78	—	95·1	—
34	111·0	65·2	88·2	91·7	79	—	87·0	—
35	123·8	64·8	95·4	96·6	80	—	81·7	—
36	119·0	59·6	96·6	92·2	81	—	86·9	—
37	121·2	62·0	94·3	—	82	—	85·8	—
38	116·3	66·8	*86·9*	—	83	—	84·8	—
39	*110·5*	67·0	100·7	—	84	—	91·7	—
40	109·9	64·0]	[87·8	—	85	—	95·1	—
41	121·0	69·8	97·1	—	86	—	96·3	—
42	120·8]	72·4	95·7	—	87	—	93·5	—
43	126·5	65·8	88·2	—	88	—	93·9	—
44	131·4	77·6	85·4]	—	89	—	90·0]	—
45	127·3	*59·3*	104·7	—	90	—	99·6	—

Italic figures denote first practice after a vacational break. [ ] denotes periods of arrested progress.

The periods are classified according to probable causes in the following section.

(*a*) *Causes apparently inherent in the learning process.*

(1) *Concentration on one component of the task.*

The component upon which attention was particularly concentrated sometimes improved, sometimes failed to improve or even deteriorated. In any case the total score did not improve.

(i) *Subject F. B.* (32–35). The subject was mainly preoccupied with learning to hold and to release the ball correctly. "I still have much to learn about holding the ball" (31). "I kept gripping the ball too hard and holding it back" (32). "Quite definitely my main difficulty is to get the ball to leave my hand in the right way" (33). At practice 34 he once got "the proper hold of the ball" and the score

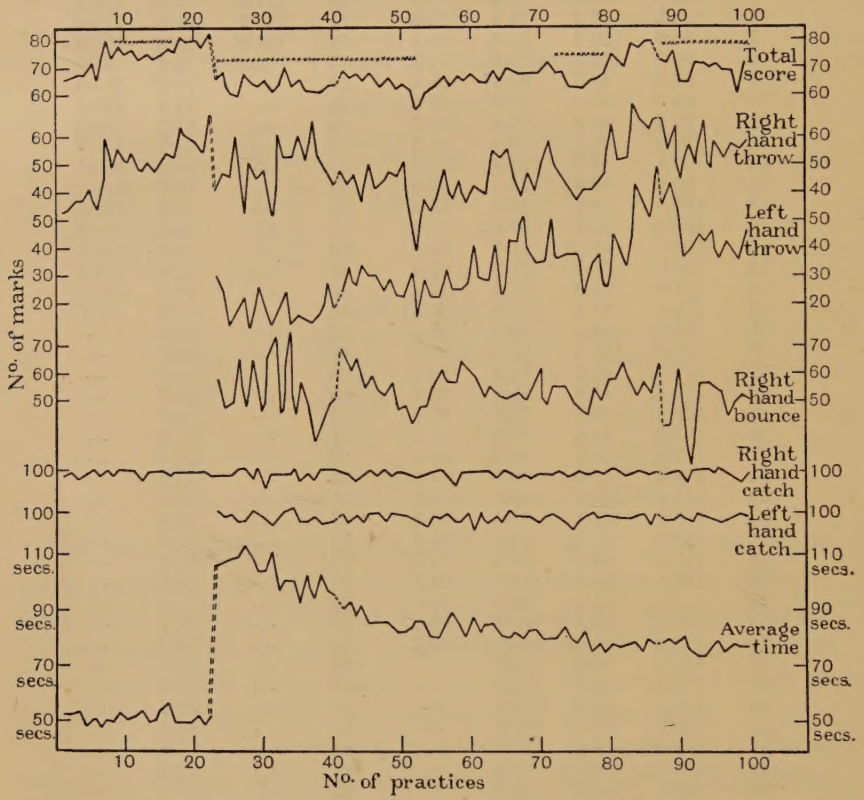

Fig. 1. Ringball, Type I: Curves of subject D. S.

× × × × × × denotes Period of Arrested Progress.
– – – – – – denotes Vacational Break.
= = = = = = denotes End of Section.

began to improve. By practice 35 he had "got into the old swing again, but as soon as I began to think about it the balls went out." After practice 35 there was no further reference in the introspections to the gripping of the ball, except once at practice 40. The score rose steeply at practice 36, although progress was not subsequently continued.

(ii) *Subject L. S.* (40–44) (Table II.) The subject resumed after a vacational break at practice 38, and decided that "the weakness of the bouncing was due to throwing the ball too hard: I must try throwing more softly" (38). Improvement followed in the right bounce, but not in the left bounce, and was accompanied by erratic scoring in the right throw. Then at practices 43 and 44 the right bounce fell

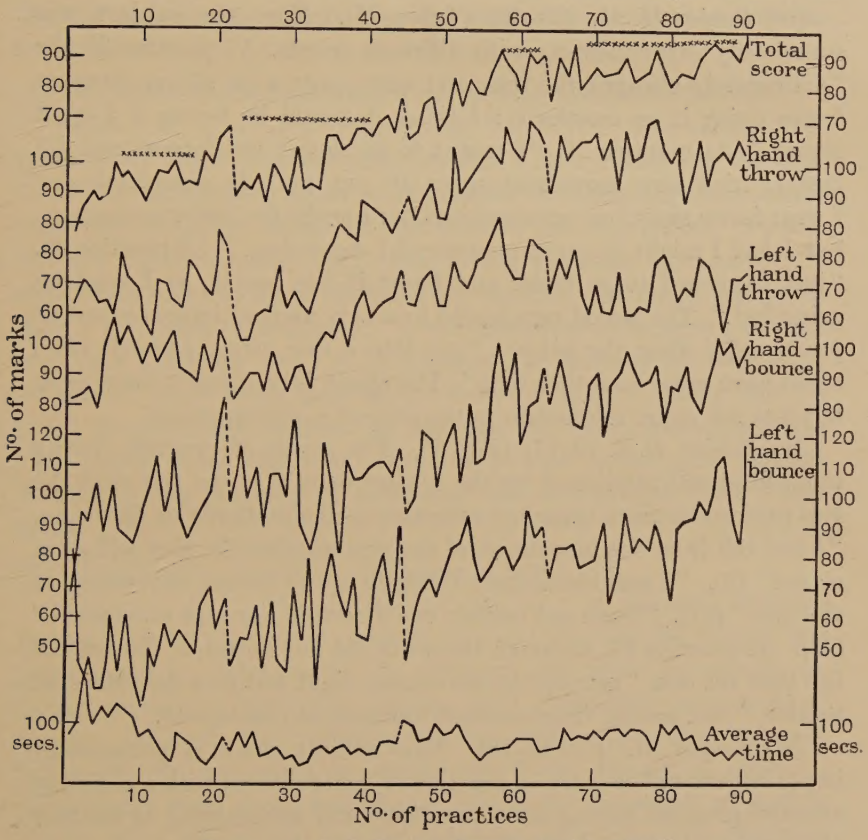

Fig. 2. Ringball, Type 2: Curves of subject M. V.
× × × × × × denotes Period of Arrested Progress.
– – – – – – denotes Vacational Break.

catastrophically; the subject was feeling off-colour and was still "throwing too strongly" (43). The period terminated with a steep rise and the subject made no further reference to the force of the throwing.

(iii) *Subject M. M.* (22–26) (Table II). The subject was apparently preoccupied with speed, which was greatly improved at practices 22

and 23, and gradually receded from this level in practices 24, 25, and 26. The total score meanwhile was practically stationary except for the very bad practice 24, where the drop was undoubtedly due to the fact that the two practices were done on the same day: "I found myself going faster and faster; aiming at the bulls made me go slower previously" (23).

(iv) *Subject M. M.* (30–32) (Table II). Here the subject was consciously experimenting with different speeds. At practice 30 she "deliberately changed the time and went quite a lot slower, because I was doing it so erratically: I think it would be better if I went slower." At practice 31, "I meant to go slower but forgot, and did better; tried slow movement again, it put me out completely, so I went faster again but remained erratic: I prefer the faster movement, but I feel I might be more accurate if I went slow." At practice 32, "just happened to go faster and found it was better, so I went on going fast." The period terminated in a very marked improvement at practice 33, when the subject "got into a nice swing and felt (she) could have gone on rather well." The speed subsequently decreased, but was not again referred to in the subject's introspections.

(v) *Subject D. S.* (8–17) (Fig. 1). The arrest of progress during this period was attributed by the subject mainly to the fact that she was paying too much conscious attention to the methods of throwing. "I can tell from the movement of the arm whether the shot will go in or not" (9). "I was consciously thinking of the tossing movement of the arm" (11). "I can get neither eye direction nor arm movement" (12). At practice 17, although the score did not improve, the subject felt that she was "getting the movement right but in a disconnected fashion," and sudden improvement followed at practice 18.

(vi) *Subject K. P.* (13–18). This subject made a pronounced improvement in her score at practice 12, and followed with a period of arrested progress lasting for 6 practices, and terminating in another abrupt rise at practice 19. This period was characterized by a preoccupation with speed accompanied by neglect of the other factors. In practice 15 there was a pronounced deterioration in both throwing and catching, although the time remained steady: the subject was "attending to speed all through." At practice 18 attention was divided more evenly between arm movements and speed, and at practice 19, when progress in the score was suddenly resumed, the subject was "not thinking much about the game." Meanwhile, however, there was a falling-off in the speed.

(vii) *Subject K. P.* (29–38). The same subject showed a period of arrested progress between practices 29 and 38. The cause of this was not so clear, since she had previously been recovering from a vacational break, and was at intervals experimenting with new methods of throwing. But the introspections indicated that she was attempting to maintain a particularly high speed of throwing achieved at practice 28, and that it was attention to this factor which arrested progress in the score.

(2) *Oscillation of attention between two or more elements of the task.*

Subject M. V. (7–17) (Fig. 2 and Table II). The subject had made fairly rapid progress until practice 7, but henceforth the total score did not improve again until practice 18. The component scores were very irregular. Attention was directed first to rhythm, "I could not get regular motion" (8); then to the muscular movements, "I sometimes seem to loose control over muscles, just could not get it right" (9); then to bouncing, "I was preoccupied because the balls were not bouncing well" (11); then to the kinaesthetic sensation, "I think I throw much more by the feel of it than by aiming with the eye." Progress was resumed at practice 18, when the subject was "not paying much attention." Practice 20 was "rather good, perhaps because I was thinking of something else and not of what I was doing."

(3) *Co-ordination impeded by conscious attention to it.*

Subject N. D. (13–29). In this one case the most plausible explanation of a period of arrested progress seemed to be that the subject was preoccupied in endeavouring to maintain and make automatic the co-ordination of a particular rhythm with a particular movement of the arm. Though often achieving the one without the other, she only intermittently succeeded in combining them, though a series of particularly good shots was always felt to be the result of such a combination. The subject realized before long that conscious attention probably impeded co-ordination, and in the end, when progress was resumed at practice 30, it was accompanied by the absence of attention and a "funny kind of dazed feeling."

(4) *Interaction between two components.*

This usually occurred at the stage when new and unpractised components were added to the task, to be performed either alternately or successively with old and more practised components. Sometimes the old component was upset by errors transferred from the new and

consequently had its progress arrested or turned into a temporary deterioration: sometimes the old movement and rhythm were transferred to the new component and so prevented it from making progress. Although such interaction was noticed by most of the subjects at these stages, it did not always arrest the progress of the total score, since progress was generally made in one part of the tests although it might be held up in another. The following are examples of non-progressive periods in the total score:

(i) *Subject D. S.* (23–52) (Fig. 1). The total score fluctuated about a level which was practically constant for the whole 29 practices, but the component curves—for right throw, left throw and right bounce respectively—showed a well-marked cycle of correlated changes as follows:

23–29: right throw weak and unprogressive.
29–37: improvement in right throw, deterioration in left throw and right bounce.
37–44: deterioration in right throw, improvement in left throw and right bounce.
44–50: improvement in right throw, deterioration in left throw and right bounce.

The relevant introspections, which were numerous and detailed, indicated that at the very outset the left hand (new movement) put out the right hand (old movement); a little later (29 onwards), the right influenced the left; at practice 37 the left hand was tending to upset the right again and continued to do so till practice 44; from practice 45 to 50 the correct movement was being got by the right hand, but this was upsetting the left hand once more. The period ended with a steep fall in all three curves—right throw, left throw and right bounce—this being the first time they had all varied in the same sense. Thereafter the scores for right and left throwing were directly instead of inversely correlated, and slow progress was made in each of them.

(ii) *Subject N. D.* (31–49). This was not strictly speaking a period of arrested progress, since a slight advance was made in the total score, but this was almost negligible in comparison with the big fluctuations of the individual curves, which again, as in example (i), showed an inverse correlation between the right throw on the one hand, and left throw and right bounce on the other, that is to say, between old (practised) and new (unpractised) components.

Between practice 31, when the new components were first introduced, and practice 37, rapid progress was made in speed and right bounce, while the right throw actually deteriorated. The subject felt she was transferring errors made in the new left-handed throw to the old right-handed throw. After a vacational break (between practices 38 and 39), there was deterioration in the new components but not in the old. Between practices 39 and 42 the right throw improved rapidly, the left throw did not improve at all and the right bounce fell away. At practice 43 a synthesis was momentarily achieved but lost again. Between practices 44 and 47 there was rapid progress in the right bounce, accompanied by a deterioration in the right throw.

(iii) *Subject F. B.* (23–26). This subject first practised with the right hand only and then, from practice 19 onwards, with the left hand only. Progress in the latter task was arrested at practice 24. At this stage the subject commented that he "seemed in rather a hurry" (23), and he had previously complained of the difficulty of controlling speed and getting a rhythm. The speed dropped slowly after practice 23. The curves and introspections indicated that the subject began with a speed and rhythm impressed upon him by the previous right-handed practice which was really too fast for the left-handed throwing, at least at this stage. Consequently progress beyond a limited point proved impossible until a new and slower rhythm had been established.

(iv) *Subject R. P.* (14–19). The arrest of progress at this stage seems to have been due to the adverse influence of the new left-hand movement on the old right-hand movement. "The inability to get shots in with the left hand seems to be transferred to the right" (17). "I find I am using the same method of throwing with the right hand as with the left; it is successful with the left hand" (15). As a result, the progress made by the left hand was counterbalanced by the poor scores of the right hand, and the total score remained stationary. Progress was resumed at practice 20, at which stage the subject commented, "I feel little difference between the two hands; both are now part of me; doing it automatically."

(5) *Components possibly not in suitable relative stages for co-ordination.*

Subject B. S. (22–42) (Table II). One very marked period of arrested progress occurring in the curve of Subject B. S. cannot be satisfactorily explained along any of the above lines.

In practices 26, 27 and 28 the subject had done badly in all parts

of the task: he was suffering from exhaustion subsequent upon practising for "putting the weight" in University sports. The old score was reattained at practice 29, and then followed the period of arrested progress in the total score. During this time the four component curves fluctuated considerably: on two occasions, practices 30 and 36, there was simultaneous deterioration in all four, while on four occasions there was improvement in three and deterioration in the other one. The relative efficiencies in the four parts of the task also varied considerably from set to set in a single practice. On a balance, the left bounce definitely improved (although it did not exceed or even reach the level of practices 21 and 22), the right bounce definitely deteriorated, the right throw possibly improved somewhat, the left throw definitely did neither. A possible explanation is that the movements could not be co-ordinated until right and left bounces had attained about the same level of efficiency—a result which was eventually secured by simultaneous improvement in the one and deterioration in the other. It is particularly noteworthy that following the vacational break with which this period closed, there was a rapid improvement in the four components of the task. The ground lost during the break was quickly made up, and all four scores rose above the highest level ever previously attained. This fact strengthens the suggestion that the period of arrest was one during which co-ordination in the separate movements was being achieved: the introspections also support this view.

(*b*) *Periods of arrested progress due to incidental causes.*

(1) *Nervous or emotional strain.*

A temporary falling off in the score was sometimes attributable to emotional factors, quite outside the game itself. In addition, there was the particular case of an emotion aroused by the game interfering with the score. This occurred in several instances towards the end of one section of the game, when the subject's score was almost good enough to allow him to qualify for the next succeeding section. Anxiety at this stage led either to nervousness or to excessive caution, with the result that co-ordination was upset and the score deteriorated. Subject F. B., practices 36–44, and Subject R. P., practices 33–42, afford examples of this factor.

(2) *Failure of incentive.*

To some extent failure of incentive operated as a contributory cause in helping to prolong periods of arrested progress originally due

to other factors, for the subject often became bored with the game when making no progress, and lost interest or the desire to improve. A falling-off in the score directly due to failure of incentive seems to have been shown by Subject D. S., practices 87–99 (Fig. 1). In this case a new incentive had unintentionally been introduced at practice 83, when the scoring was temporarily taken over by a different person for whom the subject had a great respect, and before whom she was anxious not to appear foolish. The incentive had already begun to wear off before the scorer was withdrawn at practice 90, but nevertheless, after practice 90, the old level was never reattained.

(3) *Attainment of physiological limit and subsequent 'staleness.'*

It is difficult to decide when the physiological limit in a game has been reached, but in one case at least (Subject M. V., practices 68–89, Fig. 2), there was a final period when no appreciable improvement was made for 21 practices. The case is tentatively included under the above heading, but without conclusive evidence.

(4) *Objective physical factors.*

Weather, noise, light, and similar factors were more or less obvious causes of temporarily arrested progress in certain instances.

(5) *Subjective physical factors.*

Similarly, other periods of arrested progress may be ascribed to illness, exhaustion, "incorrect body-set," etc., of the subjects.

(6) *Relearning after a vacational break.*

After a vacational break the subject often, but by no means invariably, resumed with a lower score and only more or less gradually improved. If the previous score be taken as the starting point, there was nominally a period of arrested progress, in the sense defined above, until the point at which the score again exceeded this value. The periods necessary for recovery varied much in duration from a few practices to a whole term (Figs. 1 and 2, and Table II).

(7) *Changes in the method of scoring.*

When one or more new components were added to the game, the score necessarily expressed a different achievement, and consequently the total scores before and after such a change were not strictly comparable. Thus, a temporary arrest of the score when such a modification occurred is not necessarily to be regarded as a period of arrested progress. Examples of such apparent periods of arrest are

given by Subject M. M., practices 11–20 (Table II), and by Subject L. S., practices 6–9 (Table II).

(*c*) *Periods of arrested progress in component scores.*

Most of the component scores, that is to say the separate scores for right throw, right bounce, left throw and left bounce, showed periods of arrested progress, some of which occurred while the total score was still progressing. The following are examples:

Table III. *Ringball: Periods of arrested progress in component scores.*

Subject	Curve	Practices
N. D.	Right throw	31–38
D. S. (See Fig. 1)	Right bounce	59–69
B. S.	Right throw	29–38
R. P.	Left catch	15–24

The majority of these periods of arrest seem to have been caused by diversion of attention or by interaction with some other component in the ways already described.

IV. Task B: Method and results.

The task consisted in guiding as accurately and quickly as possible, a small metal ball up to the top of an inclined plane by means of a knitting needle set in a thin wooden handle. On the surface of the plane were 21 holes, each large enough for the ball to fall through, and interspersed among the holes were small wooden barriers which it was necessary to circumvent. After each attempt the subject recorded the time taken and the spot on the board reached. Ten points were scored if the ball got to the top of the plane without mishap, while points were awarded to the other attempts in proportion to the distance up the plane reached. The game was practised by two subjects, K. P. and D. S.

Two separate scores were plotted—the first representing the average mark, *i.e.* the total number of points obtained in a practice divided by the number of attempts; the second the percentage of 'successes.' The average time occupied in the successful attempts was also recorded. The three curves, considered together, showed the varying relationship to one another of the two factors in the game—accuracy and speed.

Table IV. *Guidit. Total scores (thrice-weekly practices).*

No. of practice	Subject D. S. Average mark	Subject D. S. Average time for successes (sec.)	Subject K. P. Average mark	Subject K. P. Average time for successes (sec.)	No. of practice	Subject D. S. Average mark	Subject D. S. Average time for successes (sec.)	Subject K. P. Average mark	Subject K. P. Average time for successes (sec.)
1	1·7	—	1·8	—	29	8·6	8·0	9·4	24·0
2	1·4	—	3·3	—	30	8·6]	7·0	8·8	23·0
3	2·6	20·0	3·6	—	31	*9·3*	*8·3*	9·0	22·0
4	3·5	35·0	4·9	—	32	9·3	8·0	8·3	15·0
5	4·8	26·8	5·6	[39·5	33	9·4	8·0	9·1	19·0
6	4·9	28·2	6·1	37·5	34	9·6	7·5	8·9	19·0
7	5·7	28·9	6·1	35·0	35	9·7	7·8	9·3	19·0
8	6·9	22·9	6·4	29·0	36	[8·9	6·7	[*7·7*	*20·0*
9	6·9	23·3	6·1	38·0	37	8·7	6·5	7·3	20·0
10	7·3	16·7	6·3	34·0	38	9·4	6·5	7·1	22·0
11	7·7	15·3	6·9	29·0	39	8·8	6·8	7·5	22·0
12	8·0	16·3	7·6	34·0	40	9·3	6·8	7·6	21·0
13	7·4	14·0	7·6	39·0	41	8·9	6·5	8·6	21·0
14	7·2	13·8	7·8	34·0	42	9·2	7·0	7·5	21·0
15	8·2	12·8	7·7	36·0	43	9·4	6·5	8·1	23·0
16	8·1	11·0	7·7	35·0	44	9·1	7·3	8·9	21·0
17	8·5	10·0	7·9	40·0	45	9·3	7·0	8·8	24·0
18	7·8	9·5	8·0	36·0	46	9·1	6·8	8·7	24·0
19	8·3	9·2	8·2	35·0	47	9·4	8·0	9·0	25·0
20	8·7	7·3	8·3	32·0	48	8·9	7·8	9·0	20·0
21	[*5·8*	*8·3*	8·0	38·0	49	9·0	7·3	8·6	22·0
22	7·6	7·5	8·9	38·0	50	9·5	7·8	8·6	20·0
23	6·9	7·5	8·8	37·0]	51	*8·7*	*7·3*	8·9]	24·0
24	6·8	7·3	8·9	22·0	52	9·1	7·0	—	—
25	8·1	7·3	[*8·2*	*23·0*	53	9·5	7·3	—	—
26	8·3	7·8	7·6	26·0	54	8·9	7·0	—	—
27	8·8	7·5	8·4	24·0	55	9·2]	7·3	—	—
28	7·6	7·5	8·8]	25·0					

Italic figures denote first practice after a vacational break.
[ ] denotes period of arrested progress.

Periods of arrested progress.

There were altogether five periods during which progress in either the score or the speed was arrested. These periods occurred as follows:

Table V. *Guidit: Periods of arrested progress.*

Subject	Curve	Practices
K. P. (See Fig. 3)	(i) Time	5–23
	(ii) Marks	25–28
	(iii) Marks	36–51
D. S.	(i) Marks	21–30
	(ii) Marks	36–55

A closer examination showed that three of these, namely K. P., practices 25–28 and 36–51 (Fig. 3), and D. S., practices 21–30 (Table IV), were definitely attributable, in part at least, to breaks in practice and the subsequent necessity for relearning what had been lost. In the former

case also, the subject was probably approaching her limit of proficiency. This was still more clearly the case in D. S., practices 36–55, where the

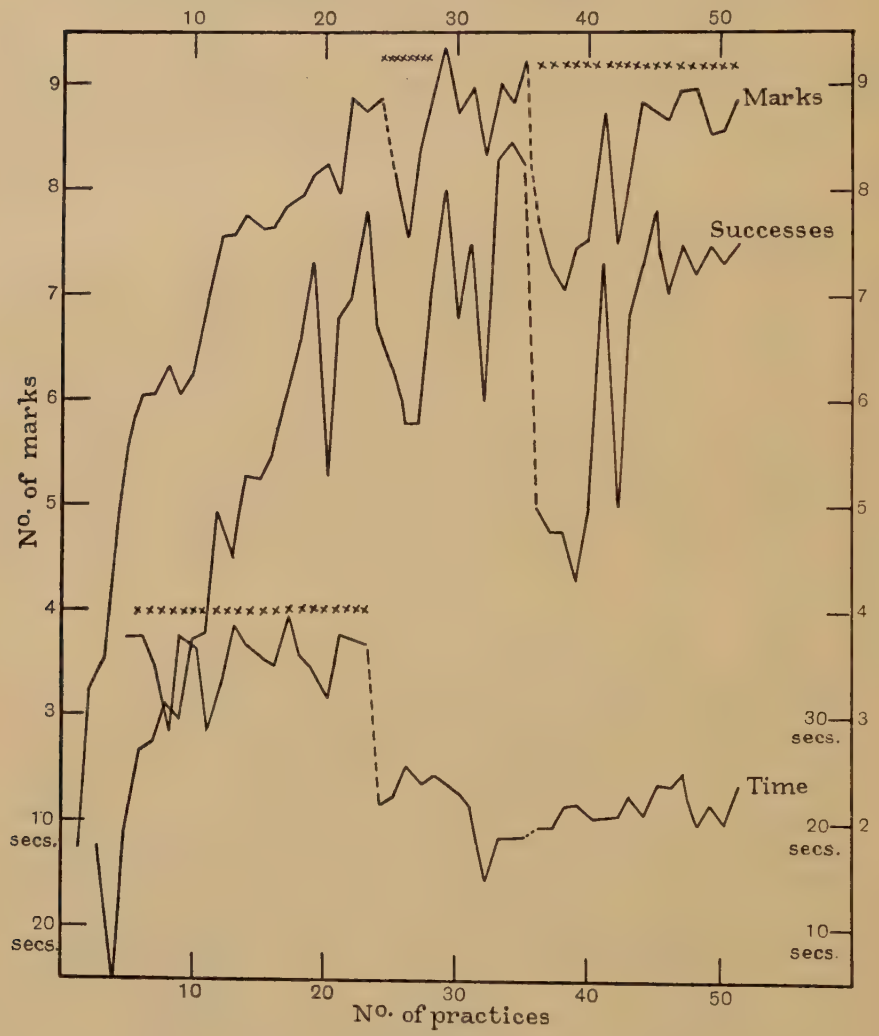

Fig. 3. Guidit: Curves of subject K. P.

× × × × × × denotes Period of Arrested Progress.
– – – – – – denotes Vacational Break.

mark-curve had become level at about 92 per cent. and the success-curve at about 85 per cent. of the maximum theoretically possible.

There remained only one other example of a period of arrested

progress, namely that in the time-curve of K. P., practices 5–23 (Fig. 3). The evidence is that progress in speed was arrested at this period because the subject was concentrating on accuracy. As regards a causal classification the period falls under a heading already discussed (p. 7) in connection with Task A, namely arrest due to concentration on one component of the task. This component improved, but the other component, at first, deteriorated.

V. Task C: Method and results.

This task involved mental as well as muscular learning, and was studied in order to compare the results with those obtained from the more purely muscular games of skill. The subjects were required to practise a phonetic system of shorthand. They were instructed in the subject-matter during the lessons which preceded the first seven practices. The actual practices consisted of three tasks:

1. *Copy test.* The subject was supplied with a reading book containing words of only one syllable, and was required to transcribe into shorthand as much as possible in five minutes.

2. *Dictation test.* This was introduced at practice 8. The subject was required to take down in shorthand a section from one of the 'fourth leaders' of *The Times*, which was dictated to him by the experimenter for five minutes, at the rate of one syllable per second, the rate determined by a muffled metronome.

3. *Speed test.* A given sentence had to be repeated in shorthand as many times as possible in five minutes. This was introduced at practice 4, and was continued until the limit of improvement had been reached.

In the copy and dictation tests one point was allowed for every word correctly written. In the speed test one point was given for each correct copy of the sentence.

The periods of arrested progress in the curves of the three subjects may be summed up as follows:

(i) *Arrested progress due to incidental factors.*

In all, 24 such periods occurred—9 in the case of K. K., 6 in the case of D. B. and 9 in the case of K. P. They were explicable as the outcome of such factors as the physical or mental state of the subject, distracting noises, relearning after vacational breaks, etc.

Table VI. *Shorthand. Total scores (thrice-weekly practices).*

No. of practice	Subject D. B.		Subject K. K.		Subject K. P.	
	Copy	Dictation	Copy	Dictation	Copy	Dictation
1	2	—	12	—	9·5	—
2	4	—	16	—	11	—
3	8	—	23·5	—	21	—
4	10	—	39	—	13	—
5	5·5	—	47	—	16	—
6	18	—	53·5	—	32	—
7	26	—	51·5	—	25	—
8	31	37	70	48·5	27	41
9	29	36	89	51	49	55
10	35	45	83	51·5	46·5	61·5
11	36	29·5	98	85	54·5	61
12	44·5	37·5	115·5	77·5	54	64·5
13	45·5	49·5	127·5	93	67	100
14	56	62	115	100	68·5	83·5
15	62·5	61	142·5	127	78·5	89·5
16	59	48	136	98·5	84·5	95
17	*47*	*31·5*	151	118·5	76	98·5
18	59	48	174·5	113	*68·5**	*83·5**
19	63	55	163·5	126	67·5	94·5
20	44	44·5	169	105·5	65	107
21	79·5	49·5	*125·5*	*107*	65	[99
22	78	59	116	117·5	80·5	107·5
23	92	73	152	124	73	106
24	94·5	72	146	126	92	106
25	89·5	93·5	159	116	98·5	107
26	118	[73·5	200	152·5	93·5	98·5]
27	123·5	89·5	196·5	150·5	99·5	112
28	138·5	86·5	207	144	89·5	117·5
29	[116	93	212·5	153	108·5	128·5
30	106·5	79	217·5	154	116	122·5
31	128	82	243	159	117	116·5
32	106·5	75·5	224	174·5	115	134
33	120	74]	*209*	*160*	127	144
34	109	97·5	216·5	159	118·5	137·5
35	119·5	86	251	166	133	151·5
36	99·5	104·5	252·5	186·5	142	[144·5
37	130]	106.5	264·5	200·5	131	136
38	141·5	[97·5	230	190	117·5	143·5
39	135·5	86·5	259·5	176	135·5	147·5
40	159·5	99	272·5	177·5	136	151
41	138	83]	276·5	180·5	156·5	146
42	150·5	109	269·5	180·5	136·5	146
43	146	97	*238·5*	*163*	159	140·5
44	159·5	132	246·5	166	159	144
45	154·5	104	269·5	197·5	163·5	145
46	162·5	99·5	281	199	126	139
47	166	96	259	219	160	151
48	183	90·5	303·5	183·5	156·5	145
49	[*130·5*	*83*	280·5	191	145	141]
50	126	99·5	260	195	156·5	157
51	155·5	122·5	296	211	163·5	149
52	163	[105·5	318·5	210	171	167
53	162·5	106·5	272·5	225·5	178	160
54	194	104·5	275	209·5	170	161
55	154·5	114	—	—	153	169
56	157	105·5	—	—	168	141
57	162·5	116	—	—	162	145
58	168	124	—	—	166	161
59	149	120	—	—	161	162
60	172·5	116	—	—	177	145
61	159	105	—	—	170	159
62	151	111]	—	—	159	148
63	176	133·5	—	—	169	158
64	176]	140	—	—	178	155
65	189	145	—	—	187	171
66	*133·5*	*124·5*	—	—	190	174

No. of practice	Subject D. B.		Subject K. P.	
	Copy	Dictation	Copy	Dictation
67	185·5	141·5	169	169
68	172	142·5	171	190
69	143	146·5	189	[187
70	174	147	178	181
71	163	142	203	177
72	189	149	[186	151
73	197	173	178	156
74	214·5	163	203	173
75	185	136·5	184	181
76	189	151	198	173
77	214·5	171·5	176	175
78	214·5	167	172	166
79	214·5	198	192]	184
80	204	163	213	170
81	222	190·5	210	167
82	219·5	189	205	156]
83	228	154	216	196
84	—	—	216	182
85	—	—	218	182
86	—	—	221	200
87	—	—	229	192
88	—	—	*157*	*157*
89	—	—	183	153
90	—	—	180	190
91	—	—	203	195
92	—	—	218	175
93	—	—	204	173
94	—	—	195	177
95	—	—	231	184
96	—	—	210	177
97	—	—	249	190
98	—	—	217	172
99	—	—	200	195
100	—	—	232	174
101	—	—	250	204
102	—	—	230	193
103	—	—	214	194
104	—	—	214	190
105	—	—	241	194
106	—	—	248	177
107	—	—	230	188
108	—	—	288	203
109	—	—	257	[196
110	—	—	268	182
111	—	—	[235	198
112	—	—	264	197
113	—	—	260	181
114	—	—	260	195]
115	—	—	262	208
116	—	—	246	206
117	—	—	241	192
118	—	—	256	207
119	—	—	256	212
120	—	—	264	188
121	—	—	244	201
122	—	—	260	206
123	—	—	258	200
124	—	—	232]	216
125	—	—	276	216
126	—	—	276	218
127	—	—	253	217
128	—	—	269	200
129	—	—	279	198
130	—	—	293	204
131	—	—	292	210

* From then onwards practised daily (except Sundays).
Italic figures denote first practice after a vacational break.
[ ] denotes period of arrested progress (inherent).

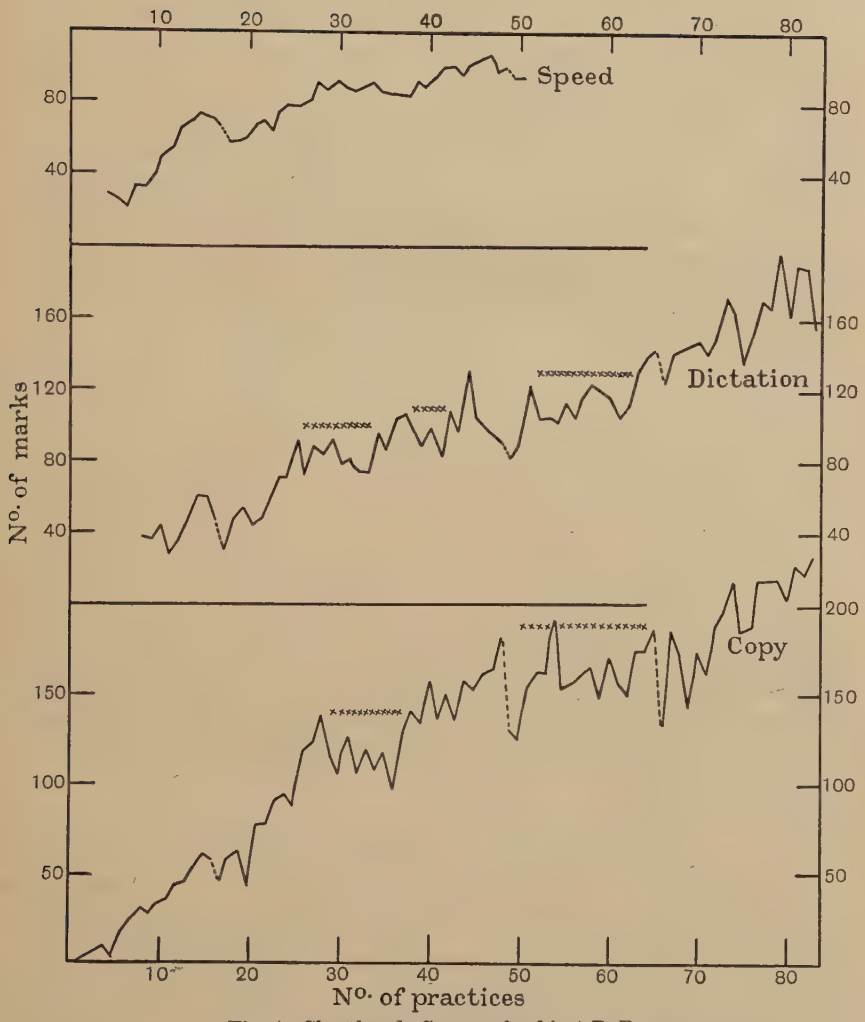

Fig. 4. Shorthand: Curves of subject D. B.
× × × × × × denotes Period of Arrested Progress (inherent).
– – – – – – denotes Vacational Break.

(ii) *Arrested progress due to factors inherent in the learning process.*

Eleven of such periods occurred—6 in the case of K. P., 5 in the case of D. B. They are shown in Tables VI and VII.

There was no obvious explanation for the arrests in K. P.'s copy-curve. The arrests in the dictation curve were probably caused by the difficulties successively met with in compounding words of two, three, or more syllables. The arrested progress in D. B.'s dictation curve,

practices 26–33 (Fig. 4), also seemed due to the difficulty of compounding long words. The progress of D. B. was arrested simultaneously in both copy and dictation tests from about practices 29–40 and again from about practices 50–62. In each case the apparent cause was a temporary inability further to increase the speed of working, when accuracy at that stage had been almost perfected. This inability was probably due to the subject's initial determination to be accurate at all costs, however slowly he went.

Table VII. *Shorthand: Periods of arrested progress (inherent).*

Subject	Curve	Practices
K. P.	Copy	72–79
,,	,,	111–124
,,	Dictation	21–26
,,	,,	36–49
,,	,,	69–82
,,	,,	109–114
D. B.	Copy	29–37
,,	,,	49–64
,,	Dictation	26–33
,,	,,	38–41
,,	,,	52–62

(See Fig. 4)

Gilbreth (1918) showed that the muscular activities of a fast movement differed from those of the same movement done slowly, and here the original neglect of the speed factor apparently resulted in an incapacity to quicken the rate automatically when it became desirable to do so.

VI. General discussion of results.

A very obvious feature of most of the learning curves obtained in these studies was the large day-to-day irregularities which they manifested. The irregularity was much greater in Ringball than in Guidit or Shorthand, indicating perhaps that the building up of a complex system of co-ordinated body movements was, for most subjects, a more uncertain and haphazard business than the acquirement of refined hand and eye co-ordination (Guidit), or of co-ordination on fairly familiar lines of mental and physical elements such as those involved in Shorthand.

The effect of day-to-day fluctuations was sometimes to conceal, sometimes to exaggerate, those periods of arrest or of actual deterioration which undoubtedly did occur in the large majority of the learning curves. It was not very difficult, however, to pick out non-progressive periods of considerable duration. Any series of six or more

practices, during which the initial score was not again reached or exceeded, has been considered a period of arrested progress demanding explanation. In accordance with this definition, some 71 periods of arrested progress were found in the records of the total scores of the 14 subjects. Of these, 43 can be more or less definitely written off as due to incidental causes, objective or subjective. Twenty-eight have been concluded to be due to factors inherent in the learning process.

In the first place it was clear that these periods often occurred when the subject was paying particular attention to one component in a complex task. This has been the case with all three tasks studied, but with an important difference. In the simpler muscular game, Guidit, and in the predominantly mental task, Shorthand, attention to one component of the task usually resulted in improvement being made in that component, although more often this was neutralized by the deterioration occurring in the other components from which attention had been diverted. In Ringball, however, although the above result sometimes followed, the characteristic effect of a concentration of attention on one part of the game was that the factor concentrated upon, equally with the others, failed to improve. Attention in this game seemed, in the majority of cases, to be definitely harmful both to the factor particularly attended to and to the score in general. The various subjects frequently stated in introspections that at the times when their best scores were made they were not paying conscious attention to any part of the game, but were "in a sort of trance," "thinking of other things," "experiencing a dizzy kind of feeling," etc. This is not to say that attention to the game as a whole, or even to the individual parts, was not in the long run necessary to improvement: on this point the data yielded no critical conclusions one way or the other. In a game such as Ringball, where proficiency obviously depends upon a great number of delicate and intricate muscular adjustments, most of them definitely outside the normal range of the subject's awareness, it may be that conscious attention to one so-called component represents only a crude interference with the co-ordinated complex, and one more likely to have harmful results than otherwise.

Two special cases of the arrest of progress in Ringball due to conscious attention may be recalled—one in which there was an oscillation of attention backwards and forwards from one component to another (p. 11), and one in which improvement seemed to be impeded by conscious attention to the process of co-ordinating movements (p. 11).

A further important group of cases occurring only in Ringball may be regarded as effects of involuntary interaction between one part of the task and the other. Presumably such interaction only occurs in a task which includes two or more parts similar in character without being identical—for example, when a given movement is made first with the right hand, then with the left. In Ringball, Type 1 it was customary for the right-hand throw to be practised and brought to a certain pitch of improvement before the other components were introduced. The result was often seen in an inverse correlation between the scores for the right throw and the left throw, and also for the right throw and right bounce. Moreover, the errors in the new movements tended to be carried over and repeated in the old movements, for example, from the left throw to the right throw. A contrary effect also occurred in some cases—the rhythm and habits of the old movements being involuntarily carried over into the new movement, for which they were not in the majority of cases suitable. For example, when a certain speed of throwing had become habitual to the right hand, the same speed tended to be adopted with the left-hand throwing when this was introduced, although it was evident that this speed was too fast for accuracy in the less practised movement.

In a wide sense all these suggested causes of arrested progress may be summed up in the phrase 'difficulties of co-ordination.' Co-ordination may apparently be impeded by undue attention to one component of the task, or by an oscillation of attention from one to the other, or even by the conscious effort to co-ordinate. Apart from attention, correct co-ordination may be delayed by interactions between the components, for example by the carrying over of errors and habits from one movement to another. And lastly, there is some reason for thinking that co-ordination may be checked when there is too great a disparity between the proficiencies in the separate components, and that in general successful co-ordination may require a certain relation between the skills reached in separate parts of a complex task.

If arrested progress is due in general to difficulties of co-ordination, it would be expected to occur only in a complex task, as was in fact the case in Batson's experiment. But in Ringball, Type I there were occasionally periods of arrested progress in the first section, confined to right-handed throwing, as well as in succeeding sections of the game. It must be remembered that the distinction between simple and complex tasks in this connection is subjective rather than ob-

jective. The real difference is between the task regarded by the subject as a unit and the task regarded by the subject as consisting of more than one component. Even the simple right-handed throwing of the first section of Ringball, Type 1 fell into the second of these categories in the case of some subjects, for they distinguished between speed and accuracy as things to be separately striven for, although speed was not included in the total score, and they consciously separated such factors as 'holding the ball,' 'force of throwing,' and so on.

The three tasks differed very considerably in the facility with which they could be split up in the subject's mind into separate components. In Ringball the separation of the task into parts was almost inevitable at first, even when, as in Type 2, the subject began his practice with the full task, and was quite inevitable when, as in Type 1, it was practised section by section. On the contrary, in Guidit and Shorthand there were no such very obvious components, although in most subjects' minds speed and accuracy were distinguished as factors which could be attended to separately, at least in the early stages. The occurrence of the periods of arrested progress reflects the difference between the tasks, for while in Ringball every subject showed one or more such periods, in Guidit one of the two subjects, and in Shorthand one of the three subjects, showed no such period, apart from those due to accidental causes. The general conclusion can be drawn with some confidence that in a task which the subject regards as being made up of distinct components or phases, there will generally be periods of arrested progress, but in a task which the subject regards as a unit, such periods will probably not occur. The arrest in progress in the first case is due to difficulties of co-ordination such as have been discussed above.

Two instances of arrested progress, both occurring with Subject K. P. in Shorthand, have not received any explanation in the text. In Shorthand there was much less possibility of analysing the score into its components, and maybe if this could be done these two periods would be found to fall into one of the classes of causes already discussed.

One other period, which must be separately mentioned, occurred in Ringball, Type 2, Subject B. S., practices 22–42 (Table II), where progress was not achieved until approximately the stage at which an initially less skilled movement had become equalized with a similar but more skilled movement (p. 13).

This suggests an interesting line for further research. Can movements separately but simultaneously learned and improving at

different rates be co-ordinated at any stage, or only at one or a few stages where there is a definite ratio of skill between them? May co-ordination be impossible at any stage, except at the very outset, if the rates of improvement are widely different ?

The periods of arrested progress did not appear to be characteristic of any particular stage in the acquisition of skill. In one instance such a period began as early as the eighth practice, and in general they appeared to be scattered promiscuously among the curves. Most of them were brief—extending over fewer than 10 practices, but periods of arrest for 10 practices or more occurred on twelve occasions in Ringball, and on a few occasions in the other games. Moreover, they did not seem to fall into two classes such as have been distinguished by Swift, Book and others, under the headings of 'short periods of arrest,' and 'true plateaus.' The duration of the periods of arrest in these experiments was anything from the lower limit fixed by definition, *i.e.* six practices, to an upper limit of 30 practices in the most prolonged case.

The form of the score curve during these periods was also not a constant and characteristic one. Sometimes it was approximately horizontal, but more often than not it showed alternating deterioration and recovery, and in a few cases almost steady deterioration. Again, even when the curve of the total score was approximately horizontal, further analysis showed that the component scores, when as in Ringball, the score was analysable, were rarely or never horizontal also. The constancy of the total score was then merely the result of the fact that the fluctuations of the component scores happened to cancel each other out. It is clear that if they cancel each other out with one method of scoring, in which they are weighted relatively to each other in a certain way, they would not do so under another method of scoring which weighted them differently. And since the weighting is more or less arbitrary, the constancy of the total score curve, when it occurs, appears to be doubly accidental.

The period of arrested progress usually terminated in an abrupt but often only temporary resumption of progress. Only in a few instances were periods of arrested progress followed by prolonged periods of accelerated progress. It is noteworthy that the longest of all the periods of arrested progress, that of Subject D. S., practices 23–52 (Fig. 1), was not followed by prolonged or greatly accelerated progress.

The plateau as a definite single phenomenon with a single cause seems to become more elusive the further it is pursued. What actually

appears to occur, in the tasks here studied, is rather a whole series of arrests of progress, some brief, some prolonged, some due to outside causes, and some to causes inherent in the learning process, some eventuating in accelerated progress and some in a mere resumption of the old rate.

The writer's thanks are due to Mr F. C. Bartlett for much helpful advice in the planning and carrying out of these experiments.

VII. Summary.

The curves of learning in three selected tasks were found to show, in addition to the daily fluctuations of score, various periods of greater or shorter length during which progress, as measured by the score, was completely arrested.

About 60 per cent. of these periods could plausibly be accounted for by accidental factors, the remainder were apparently inherent in the learning process itself.

Analysis of the score curves and a study of introspections indicates that progress was arrested in one or other of two sets of circumstances:

(*a*) When the subject's attention was consciously centred upon one component in the task. Sometimes in this case the factor upon which attention was concentrated failed to improve, at other times this factor improved, other factors simultaneously deteriorating.

(*b*) When there was an involuntary interaction between two or more components, so that habits were carried over from one to the other. This usually occurred when new components were introduced side by side with older and more practised components. Sometimes the errors of the new movement were carried over and repeated in the old movement, at other times the rhythm and habits of the old movement were carried over into the new movement; to which they were not at this stage suited.

In a wide sense all these causes of plateaus may be summed up in the phrase 'difficulties of co-ordination'—a successful co-ordination seeming to involve a certain distribution of attention and a certain ratio of skill between the individual components.

Periods of arrested progress tended to occur when the subject regarded the task as made up of separable components, but not when the task was regarded 'as a whole.'

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PSYCHOLOGICAL TESTS AND ACCIDENT PRONENESS.

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I. Introduction.

A great amount of psychological data that has been gathered in the past 20 or 30 years has had little application outside university laboratories. This application has been almost solely in the field of education, but there is a growing recognition of the need of psychological methods in industry and an increasing number of practical applications. The great need for consideration by trained psychologists of the personality and ability of individual men has been almost entirely lost sight of in large companies. The present study was undertaken after a careful survey of all relevant work upon which there was available information (1), and the reported investigation built on such work as was applicable.

The work was undertaken for the Boston Elevated Railway, in which 2300 men are employed in the actual operation of surface cars. Although their accidents compared very favourably with other American street railway companies, the management felt that something more could be done to control the accidents, and caused this investigation to be undertaken in 1927. The definite aim was reduction of accidents and, particularly, reduction in collision accidents.

Analysis soon made it clear that a considerable proportion (55 per cent.) of accidents were caused by a relatively small percentage (27) of the men. It was decided that a large reduction in accidents could

be effected if the accident frequency of these men was brought down to the level of the remainder of the men.

As the main subject of the research, 472 men (called for convenience 'high accident' men) were selected and specially studied. Each man was thoroughly studied; his accidents, their nature, etc., his health, medical condition, personality, operating habits, etc. He was then treated in such manner as seemed desirable. Diagnosis in most cases was clear, but in some cases it was very difficult to determine the reason for a man's accidents. It was suspected there might be difficulties in reactions which psychological tests would reveal.

II. Testing laboratory.

Tests were therefore designed and a testing laboratory set up. The types of tests used were determined partly by type of accidents and partly by necessity of testing factors as general as possible, that is to say, not specific to the laboratory. For the purpose of trying out the tests, it was originally intended to take 100 men, 50 high accident men and 50 low accident men. These numbers, owing to absences and so forth, were reduced to 43 of each class. The men tested were motormen varying in age from 27 to 72 years. They were taken from one division so that conditions of discipline and operation could be as similar as possible. In the testing of the men the employment manager, not a trained psychologist, gave the men their instructions under the supervision of the experimenter. This was for the purpose of making the men unstrained in their attitude.

While it was desirable to make the tests appear as practical as possible to the men, it was considered that the actual use of street car machinery in the tests was unsuitable because of the gross muscular movements involved, which were likely to obscure the functions to be tested. Nevertheless, it was not impossible to have the tests appear practical to a certain extent and obtain the interest of the testees. The tests were mainly of simple psychological functions rather than of complex practical situations.

III. Description of tests.

These were the factors it was decided to measure: perseveration, oscillation, speed, accuracy and muscular control.

Perseveration was measured in four tests.

(1) Choice reaction test. In the first test the subject was instructed to respond to a red light by pressing a button with his right

hand, and to a green light by pressing a button with his left hand. The test was then repeated, the subject responding to the red light with his left hand, and to the green light with his right. The differences between the number of omissions and mistakes and the speed of reaction in the first and second tests was used as two measures of perseveration. If these differences were zero, or showed that the second test was performed more quickly or accurately than the first, the subject was considered a non-perseverator.

(2) The tapping test as used by Lankes was adopted(2). Two 15-second periods of tapping were counted, and the average taken as the perseveration score. The test, possibly owing to the misunderstanding of instructions to the motormen, proved unsatisfactory as will be shown in the correlation table.

Two other tests were given, but could not be included in the final results owing to mechanical difficulties in the apparatus. The flicker wheel was used(2). Discs of red and green, yellow and blue, orange and slate, and black and white, were used with the various improvements of Lankes. An electric motor was used rather than turning the wheel by hand in an effort to obtain uniform results. Correlation of the test was zero and, owing to mechanical difficulties and misconceptions on the part of the men which were hard to overcome, it proved itself unsatisfactory.

On the dummy control the subject was instructed to swing the control handle around in 9 seconds, pausing a second in each of the eight notches. Then the resistance was raised on the shaft of the handle, making the handle harder to turn, and the test repeated. This was carried out at a still greater resistance and, finally, at the first resistance. The subject was given two trials at each level of resistance. A comparison of the averages of the scores on the two trials at each degree of resistance was intended as a measure of perseveration. As this test was not given to all the men, these scores could not be included in the final results.

Oscillation was measured by variations of the speed in two tests and accuracy in the third.

(1) Oscillation in speed of simple reaction. The subject reacted to an irregular flashing of a green light by pressing a button with the forefinger of each hand. The degree of variation in speed of reaction in the test with the right hand was the score used.

(2) Oscillation in speed of choice reaction. The test as it was given for perseveration the first time was scored for variation in speed of reaction time.

(3) Oscillation in accuracy, the six-light test. There were six lights which flashed one at a time in varied sequence on the table in front of the subject, to which he responded by pressing the button corresponding to the flashing light. The period of testing was divided into six parts, and the variation in errors in these parts taken as the measure of oscillation.

Speed of reaction time was measured in four tests, three being reactions to light and one to sound.

(1) Speed of simple reaction to light. The simple hand reaction to light test as used for oscillation was scored also for reaction time. A complete test was made on both the right and left hands, and the medians of the two averaged and taken as the speed of simple hand reaction to light.

(2) Speed of choice reaction to light. The choice reaction test as described above was used. The median length of reaction time was taken as this measure.

(3) Speed of complex reaction to light. A dummy street-car controller was set up in the laboratory on a platform. On the wall in front of the subject standing at the controller were four lights, red, yellow, green and blue. The subject stood on the platform facing the wall, with one hand on the brake handle and one hand on the control handle. As the lights flashed the man was instructed to respond to them in a definite manner. The instructions followed the habitual reactions to subway lights. Owing to the mechanical clumsiness and lag in recording, the reaction times were so inaccurate as to be of little use and no record was preserved. It was, however, decided to keep this test and give it as an introductory one, since the men were highly interested in it and it formed the link between their street car operation and the laboratory tests.

(4) Speed of simple reaction to sound. In this test a series of noises including the sounding of a street car gong were given as stimuli, and the subject required to select and react to the noise of the street car gong only. It might well be called a sound discrimination test. It was performed with both right and left feet. Correlations of the reaction times under these conditions with speed of simple reaction to light stimuli was high (0·87), and the results were not used in correlation.

Accuracy was obtained in three scores.

(1) Number of omissions in the simple hand reaction to light test.

(2) Number of mistakes and omissions in choice reaction to light.

(3) Number of omissions and mistakes in the six-light test.

To measure *muscular control* the subject swung the controller handle through the eight notches in 9 seconds, taking a second to each notch. It was proposed to use the time interval between each notch in two trials as a measure of steadiness and ability to complete the task in 9 seconds.

In all these tests, with the exception of tapping and the flicker wheel, the stimuli were automatically set off and the responses automatically recorded (3).

IV. Reliability results.

The first step was to discover whether the scores of the test could be regarded as reliable; *i.e.* did they measure the functions they were intended to measure? In view of the expense and difficulty of repeating the tests on the same men, the criterion of reliability was taken as inter-correlation of the tests. It is obvious that tests which are in themselves unreliable cannot possibly correlate with other tests. This inter-correlation is shown in Table I.

Table I. *Inter-correlation of tests.*

($N = 86$.)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(1) Speed Simple reaction	—	·45	·48	·39	·37	·75	·34	·05	·06	·03	·02
(2) Speed Choice reaction	·45	—	·32	·40	·31	·46	·44	·16	−·05	·04	·002
(3) Accuracy Simple reaction	·48	·32	—	·43	·47	·44	·23	·02	−·05	−·07	−·001
(4) Accuracy Choice reaction	·39	·40	·43	—	·43	·37	·41	·01	−·03	−·26	·24
(5) Accuracy Six-light test	·37	·31	·47	·43	—	·37	·32	·17	−·03	−·08	·21
(6) Oscillation—Speed Simple reaction	·75	·46	·44	·37	·37	—	·53	·06	·12	·05	·01
(7) Oscillation—Speed Choice reaction	·34	·44	·23	·41	·32	·53	—	·13	−·07	·02	·07
(8) Oscillation—Accuracy Six-light test	·05	·16	·02	·01	·17	·06	·13	—	·06	−·16	·07
(9) Perseveration—Speed Choice reaction	·06	−·05	−·05	−·03	−·03	·12	−·07	·06	—	·29	·11
(10) Perseveration—Accuracy Choice reaction	·03	·04	−·07	−·26	−·08	·05	·02	−·16	·29	—	·001
11) Tapping	·02	·002	−·001	·24	·21	·01	·07	·07	·11	·001	—

The following points on this table are to be noted:

(1) Four tests, oscillation in six-light test, perseveration as measured by speed and accuracy and by the tapping test were so low

in their inter-correlations (average 0·04) that they were regarded as so unreliable as to be disregarded.

(2) It was originally intended to discover whether oscillation and perseveration showed up as group factors. Owing to the fact that all the perseveration tests proved unsatisfactory and one oscillation test, it is impossible from the remaining significant correlations to determine anything about the presence of these group factors.

(3) The remaining seven tests show an average inter-correlation of 0·42.

(4) Application of the tetrad differences formula gives a maximum tetrad difference of 0·10 with probable error of 0·03.

(5) This shows the rather surprising result that among these different tests of reaction there is nothing but a general factor and specific factors. The nature of this general factor is, of course, entirely unknown. It may possibly be called 'reactibility' or some such function. It may or may not be related to intelligence. It is hardly likely to be solely intelligence.

(6) Correlations of each test with general factor are shown thus(4):

Correlation of	(1)	with '*g*'	... ...	0·72
,,	(2)	,,	... ...	0·63
,,	(3)	,,	... ...	0·58
,,	(4)	,,	... ...	0·60
,,	(5)	,,	... ...	0·58
,,	(6)	,,	... ...	0·65
,,	(7)	,,	... ...	0·63
,,	sum of tests with '*g*'		... ...	0·89

This correlation (0·89) of the sum of tests with the general factor indicates that they have a satisfactory reliability. Assuming that they are satisfactory measures of whatever the general factor is, then the next question is whether this has any relationship to accident tendency.

V. Psychological reactions as one of many causative factors.

A man is regarded as accident prone if he has more than a given number of accidents, that number being determined in relationship to the average accidents of the group. The work which has been done in determining the causes of accident proneness has shown plainly that many factors may either singly or in combination lead to this condition. Such factors for instance as health condition, attitude towards job, amount of time worked, hazards of route, home circumstances, luck, etc., as well as abnormalities in psychological reaction. Thus, as

psychological reactions are only one of a number of factors leading to accident proneness, it cannot be expected that a correlation of psychological test results with the number of accidents can be very high. In fact it may be said that if such a high correlation were obtained, such a result could not be regarded as reliable. For instance even Farmer, using the best out of a large battery of tests, could not get a higher correlation of test results with accidents than 0·289(5). The correlation here is of about the same order (0·20).

The effect of the inclusion of uncorrelated factors in lowering correlation between two variables may be shown by the following formula:

$$r_{(x)(x+a+b+\ldots n)} = \frac{1}{\sqrt{n}} (6).$$

Thus, if we regard x as psychological test result and

$$(x + a + b + \ldots n)$$

as being accident proneness, then the size of r depends upon the number of other factors such as a, b, ... n. For instance, if we assume three other factors such as (a) health condition, (b) hazards of route, and (c) number of hours worked, and that these are uncorrelated and of equal weight, then even if the psychological tests are perfectly reliable, r could not be greater than 0·50.

Because of the presence of these various other factors the low correlation coefficient cannot, therefore, be regarded as a suitable measure for determining the relationship between test results and accidents.

VI. Relationship found.

Two methods for determining the value of the tests may be used.

In selecting the subjects for testing an equal number of high and low men were taken. In view of the fact that men vary considerably in the amount of operating they do, age, length of service, hazards of route, etc., it was considered advisable to select them in pairs, one high and one low, in such manner that the members of each pair would be alike as far as such conditions were concerned. That is to say, the two members of each pair would be approximately alike in regard to age, length of experience, difficulty of routes operated, etc., but different only in their accidents.

First method. Each high accident man was then studied in relationship to his mate to determine the conditions which might

explain his accident proneness. By this means the influence of senility, health, experience, etc., was in large measure eliminated. Thus, psychological condition would not be given as a cause of the accident proneness of a man unless his test record was poorer than that of his mate. The results of these comparative diagnoses are shown roughly classified below.

Table II. *Comparative diagnosis of accident proneness.*

	Major factor	Minor factor	Total
Psychological tests	6	17	23
Health and medical report	16	4	20
Relations to job	8	12	20
Other	13	1	14

This table shows that the psychological tests did differentiate the proneness to accident. It also serves to illustrate the presence of other factors, and the consequent fact that a correlational relationship would be attenuated by their presence. It will be seen that in the majority of cases more than one factor contributes (in fact in some cases three factors might be present).

It will be seen that in only 23 cases out of 43 was the psychological test record of the high accident man the poorer, leaving 20 cases in which the good operators made equal or poorer test records. This fact, while again it shows the reason for low correlation, does not minimize the value of the tests.

The main object of the tests was to assist in the diagnosis of accident proneness. In so far as in 23 cases the test results have thrown light on the behaviour of the individuals concerned they have been valuable. In this respect low total test scores just serve to point to the subjects whose reactions are peculiar. Individual analyses of test results then follow.

It may be stated that there is no significant correlation between any of the four factors given above.

Second method. The 86 subjects were divided into three groups, *A*, *B* and *C*, according as their test records were good, medium or poor. The number of high and low men in each group was then found with the following result:

Table III. *Comparison of test score with accident record.*

Grade on test	High accident	Low accident
A (good)	16	20
B (medium)	19	21
C (poor)	8	2

This table again illustrates the fact so commonly found in the application of psychological tests to industrial situations—namely, that those who are very poor in the tests are the ones who prove the value of the tests—a fact which correlation coefficients hide.

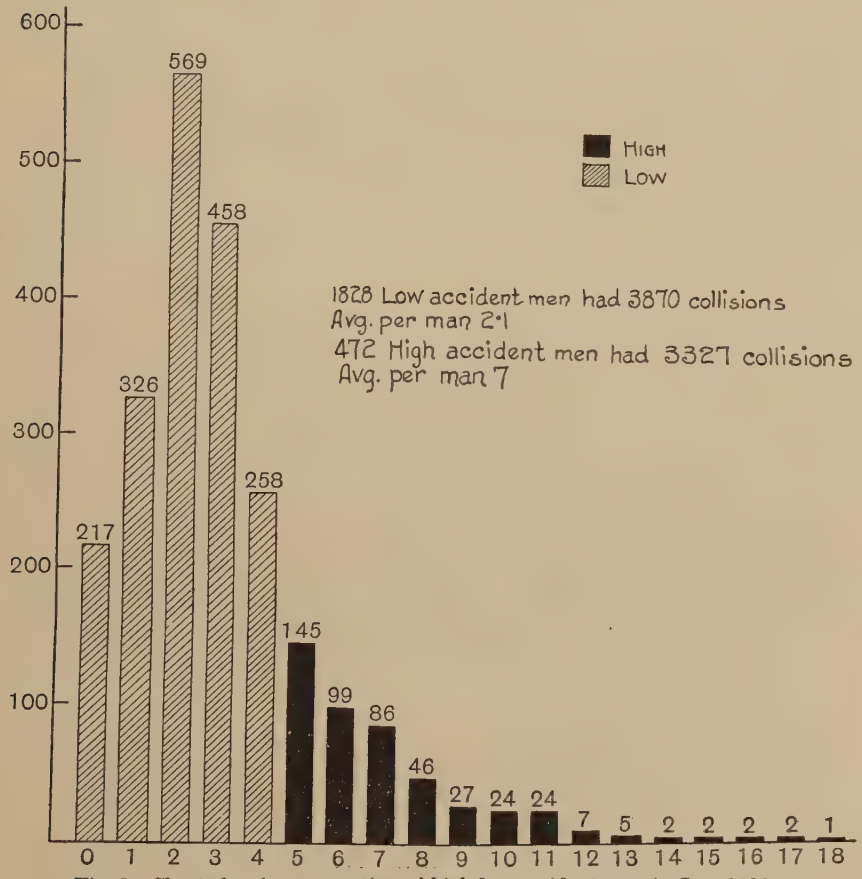

Fig. 1. Chart showing proportion of high-low accident men in Jan. 1928.

The differentiation between high and low accident men of the Grade *C* testees shown above is quite sufficient to indicate that the tests used are related to accident proneness.

It is suspected that coarseness of grouping in regard to age, experience, etc., has influenced these results, so that further work projected will be done with more homogeneous subjects. It is considered, however, that the results herein outlined are sufficiently encouraging to warrant the testing of a further larger group.

VII. Summary and conclusions.

1. The difficulty of diagnosing the causes of accident proneness led to the design and set up of psychological tests, mainly of reaction.

2. Of eleven tests seven proved reliable, correlating 0·89 with '*g*.'

3. No group factors were found present, but only '*g*' and '*s*.' The nature of this '*g*' is unknown.

4. The ordinary correlation coefficient shows no significant relationship between test results and accident record.

5. Two other methods, however, show the value of the tests in diagnosis and the relationship to accident proneness of extremely poor test performances.

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PSYCHOLOGICAL TESTS IN RELATION TO EDUCATION AND VOCATIONAL GUIDANCE[1].

By SHEPHERD DAWSON.

I. Neglect of practical application of psychological tests in Great Britain.

It would be an exaggeration to say that psychological tests have played any significant part in education as it exists in Great Britain to-day. Their principal service, apart from the diagnosis and measurement of mental deficiency, has been to bring home to the mind of all interested in education the fact that children differ in innate mental capacity and that such capacity is distributed like any other natural character. Even in measuring mental deficiency the medical man, who alone is permitted to certify such cases, puts little trust in them, preferring to rely on his own general impressions.

The recognition of variations in capacity may appear a trivial accomplishment, but it marks the beginning of a new outlook in education. To appreciate its importance one has only to remember that last century it was the practice of the Board of Education to allocate grants to primary schools on the number of children who passed the annual examination, the assumption being apparently that all children were capable of a scholastic goose-step and that the teaching was to blame for any failure to keep in line. That practice has been abandoned because it was unjust and unworkable, and the results of psychological testing have confirmed the wisdom of the change and have shown the futility of imposing a rigid curriculum on all and the need for studying children and making the most of their potentialities.

These tests have hardly been used at all, except experimentally, for purposes of school organization in this country. Practically the only tests that have proved useful have been the tests of general intelligence. Tests of imagination, reasoning, attention, memory and all those mental functions which figure in books like Whipple's well-known volume, are at present little more than laboratory curiosities, except in the hands of the vocational psychologist, and perhaps he too is no exception. Nobody takes seriously their immediate results

[1] This paper was read before Section J at the South African Meeting of the British Association, July, 1929.

as tests, not even the psychologist. It is practically impossible at present to make any clearly-cut statements of practical value about ability to reason, to attend, etc., from the information provided by using these tests. The records of intelligence tests have been analysed so as to show how intelligence is distributed, how it varies with social position and other conditions; they have yielded a mass of information and raised a crop of problems: but nothing corresponding to this has arisen from the use of other psychological tests.

II. Reasons for this neglect.

This is due partly to lack of interest on the part of the administrators, partly to the difficulty of finding suitable tests, and partly perhaps to the policy of psychologists, who, realizing the tentative character of the tests and the difficulty of interpreting them, have deprecated their use by any but themselves; but the fault lies mainly with the tests themselves and particularly with the psychology that lies behind them. They smack of quackery. Many of them are published without any indication of either norm or probable error, so that one may use them without knowing the significance of the results obtained. Even when norms are given, they are often based on so few records that they are obviously useless. Sometimes this may be due to a desire on the part of their authors to prevent their misuse, but more often it seems to be the result of ignorance of what a test *is*. To print a set of more or less interesting questions and give them a psychological name is not to invent a psychological test: they must test some psychological function.

Essentially, a psychological test is some short problem or set of problems given, not for themselves, but for the purpose of indicating capacity or ability; for example, the ability to memorize a piece of prose, a list of words or nonsense syllables *might* be taken as a measure of the ability to memorize other things (actually, it would be a very unreliable measure). In some respects it is like the sampling done by the analytical chemist when he examines a cargo of ore or by a prospecting geologist when he makes a boring.

III. Capacity and ability.

Here I should like to make a distinction between ability and capacity. Capacity is a matter of natural endowment; it is aptitude, talent, potential ability. Ability implies more; it implies capacity and in most cases experience and training: it is actual; it is what one can

do here and now. Capacity may exist without ability, but not ability without capacity. One may have the potentialities, the capacity, of a mathematician without his ability.

Every test must be primarily a test of ability; but psychological tests claim to test capacity, and therein, it is said, lies their peculiar virtue. Scholastic examinations are more obviously tests of ability, but they are often considered to be tests of capacity; for example, many educationists regard the ability to pass a classics examination as a proof of capacity for dealing with any of the intellectual problems of life.

Although psychological tests are commonly believed to test capacity, there is no reason why some of them should not be explicitly recognized as tests of mental ability. If the ability to reason along certain lines, to memorize certain kinds of material or to observe objects of certain classes can be developed by training—and we all believe they can—and if these abilities can be tested by short tests—which is possible—then such tests are as much psychological as any others.

IV. METHODS OF ESTIMATING CAPACITY.

It is when a test claims to discover and measure capacity that we come up against difficulties. There are several ways in which what is really a test of ability *might* become a test of capacity.

(1) If one can be quite certain that a test is new to all the examinees, then it seems reasonable to suppose that success at this test can be determined only by innate capacity. An example would be a test of musical capacity applied before the examinee had had any sort of training. It is, however, difficult to find such tests, perhaps impossible. Further, if such a test is to be of practical value, we have to assume that people of like capacity are equally able to profit from the same training; but it is just possible that they are not, that they differ in trainability. If they do differ in this respect, then a test of this kind will be of value only if it be accompanied by a test of trainability. This suggests a second method.

(2) The examinees might be given complete training and then tested. Here we assume that differences of ability revealed under these conditions depend solely on differences of capacity. For practical purposes of prognosis this method is obviously useless: but it would be useful for checking the results of other methods.

(3) Finally, the test may be of such a kind that it can be assumed that all the examinees have had equal training or equal opportunities of training. The Binet tests of intelligence are the best example of

this kind. Binet put to children a number of questions about topics which were likely to come within their everyday experience, and assumed that those who picked up the necessary information or acquired the necessary skill at an earlier age than the average child were bright or intelligent, and that those who were slow in doing this were dull: and subsequent enquiry has confirmed his assumption. The danger here lies in variations of opportunity and training. Obviously, a child who has not had the opportunity of using the current coinage, or of buying and selling, or of learning to read and write, is at a disadvantage when he faces the Binet tests. The danger, however, may not be so serious as it appears at first sight, for the social environment of children living in civilized communities differs very little.

Until these methods have been compared, the use of psychological tests must be attended by some uncertainty. The third is the one which has proved most successful hitherto, partly because it has been used more extensively than either of the others, and partly because it has been used with children whose educational training is remarkably uniform (up to the age of 9 or 10 the schooling given to children of all classes is very much the same).

We are specially in need of sets of observations made on the same individuals over a long period of time, for it is only by such observations that one can measure the validity of a psychological test as a measure of capacity. Validity must be distinguished from what is commonly called 'reliability.' Gauging the reliability of a test generally means finding whether it gives consistent results when it is repeated on the same individuals at intervals of a few days. 'Consistency' might be a better name for this. Consistency is not the same thing as finding whether a test measures capacity, for success at a test may depend on some special ability which has been acquired, and, as a method of measuring that ability, it may give quite consistent results without indicating capacity.

Validity can be established only by showing that the relative quality and quantity of the work done at a first performance correlates closely with subsequent work of a similar kind. Comparatively little work has been done on this particular problem, and most of it has been in connection with tests of general intelligence. Several investigators have made a series of observations on the same children extending over a year or more for the purpose of finding whether the I.Q. is constant, and this ratio does measure, or at any rate claims to measure, capacity. Unfortunately, the intervals between the testings

have usually been very short, most of them a year or less. They are, however, sufficiently numerous to produce the conviction that the I.Q. is approximately constant during school life.

The common practice of correlating success at vocational tests with success during apprenticeship, as judged by a foreman or manager, is a step in this direction, but usually a very short and uncertain one: the establishment of a correlation between success at a test and a foreman's judgment on an apprentice's ability does not prove the presence of a specific native capacity. Until enquiries like those into the constancy of the I.Q. have been made with other psychological tests, it will not be possible to speak with certainty of their validity as tests of capacity.

V. CONSTRUCTION OF TESTS.

As regards the principles on which the tests have been selected or constructed, some of them seem to be the issue of an accident, others are obviously the result of introspective analysis or popular psychological distinctions. The so-called sample tests, analogous tests, empirical tests, and tests of elementary sensory capacities all imply a certain amount of introspection. The origin of a test is really a matter of little importance; it does not affect its usefulness. Introspective analysis of it is useful, but only as an indicator; the real test of a test is pragmatic—its usefulness—the way it works. This is shown very clearly by intelligence tests. As intelligence is a popular word with an ill-defined connotation, a good intelligence test must grade people in much the same way as they are graded by a competent observer: it is, however, less subject to individual opinion; it is more precise and speedy.

The invention and use of psychological tests has been much hampered by the old 'faculty' psychology, which is still popular in one form or another. The terms used in naming them, intelligence, memory, attention, etc., are embarrassing; they suggest entities which are to be measured, whereas what one really tries to do is to examine and record the results of the subject's behaviour when he performs tasks in which he is supposed to be remembering, attending, or acting intelligently. It would make for progress if we could frankly abandon these popular notions and concentrate on the ability to cope with certain situations. Then we could make out an intelligible programme of enquiry: we could ask how soon the abilities appear, whether they remain constant, whether and how they can be developed, whether they are correlated with one another and with vocational success,

whether they are the same in parent and child, how they are distributed among the population, and so on.

There is still much doubt whether the intelligence of adults can be tested as can that of children. It may be true that the average mental age of the American Army as measured by group tests was not more than 13 years, but it is possible that the tests did not really tap their intelligence; for the situations they presented were so strange to the soldiers that the latter could not bring to bear on them what intelligence they had. We have tried in Glasgow, but so far in vain, to find paper tests which are applicable to adults of the labouring classes. Fathers and mothers of these classes get into such a state of excitement at the sight of paper and pencil that they are quite incapable of responding sensibly: one of them remarked, "You should ask our wee Johnnie to do this: he's at school." Their mental age, as measured by such tests, is almost invariably lower than that of their own children. It is, of course, possible that the race is improving, but not at this rate; it is more probable that, having been so long away from school, these people have lost the scholastic habits of thought which are necessary for success in these tests. It is not even certain that these tests are very reliable even with educated adults, for the correlation of their results with scholastic and academic success is often very low.

VI. Their practical value.

So far we have been considering psychological tests from the impersonal, scientific, and logical point of view; but they have their practical, clinical side. Practice involves more than logic: it involves insight, imagination, and the ability to 'sense' the effect of subtle conditions—what is commonly called 'intuition.' While there is much to be done to put these tests on a firm scientific footing, their usefulness is beyond question. Mental testing is like a good deal of medical practice: it is practical and empirical rather than scientific; it is a useful makeshift which must be retained until something better appears.

General intelligence tests have come to stay: no one who has used them seriously questions their usefulness. The others are, on the whole, at present at any rate, more useful for what they suggest than for what they actually tell you. This is particularly true of performance tests, as one may gather from Earle's *Report on the Use of Performance Tests of Intelligence in Vocational Guidance*. After reading this report one is left with the impression that, as tests of intelligence, they are very imperfect, that they test special abilities, and that their prin-

cipal use in vocational guidance is the opportunity they give to an acute observer for forming an impression of the temperamental traits and general behaviour of the examinees.

The same impression has been given by the use in clinical work not only of performance tests, but of tests of more obviously specific abilities, like memorizing, forming associations (substitution test), and quickness of perception (cancellation). In the Glasgow Educational Clinic, where children are examined who have been referred to it on account of abnormalities of behaviour or inability to make normal progress in one or more of the school subjects, it has been our practice to make fairly full case histories and to supplement these by the results of psychological tests. The most generally useful of these is the intelligence test. Often nothing more is required: sometimes it shows, for example, that a child's backwardness in his school work is just the consequence of a natural dullness, which has escaped the attention of the teacher, especially when the child is docile and hard-working. Frequently, misdemeanours, such as bullying and truancy, are the obvious result of inability on the part of the child to keep pace with his fellows. The general intelligence test provides an opportunity for examining many aspects of a child's mind and for making observations on his temperament and general behaviour. The special tests, *i.e.* tests of special abilities, are useful for following up suggestions gained in the course of the more general examination.

Mental tests do not yet give a picture of the human mind that is at all comparable with the picture given by a set of physical and physiological measurements. There seems, however, reason to hope that when they have been applied extensively and their results subjected to careful statistical and psychological analysis, they will take their place with such measurements. Then they will form an integral part of our educational system, and it is possible to some extent to predict the part they will play. They will play an important, but not very considerable part, for they will concern problems of organization rather than of teaching, and teaching is, after all, the biggest thing in education. Their importance will be most obvious at, but not entirely confined to, the beginning and the end of a child's school career. For purposes of classification they will supplement scholastic tests, and by showing what can be expected of a child they will give a fairer estimate than is now possible of the work of both teacher and pupil.

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A NOTE ON THE RELATION OF CERTAIN ASPECTS OF CHARACTER TO INDUSTRIAL PROFICIENCY.

By ERIC FARMER.

During an investigation for the Industrial Health Research Board I watched several hundred boy apprentices do certain psychological tests, and I endeavoured to judge some of their characteristics by the way they did them. I quickly found that differences in the methods of doing the sensori-motor tests used in the investigation were not sufficiently marked to be the basis of a character assessment, and so I confined myself to judging the subjects' characters by the way they did a performance intelligence test (cube construction). The object I put before myself was to note the various ways the boys did the test, and to learn from that the proper categories to divide them into, and I definitely avoided using any preconceived psychological notions of my own.

The first group contained 259 boys, and I made very full notes of the methods employed. When these were analysed, it was found that they were far too varied to permit of their being grouped into well-defined and exclusive categories so that they might be compared with the boys' subsequent trade proficiency, which was the object of the experiment. Certain definite characteristics showed themselves, however, and it was decided in future to pay attention to these only, and to divide the subjects into exclusive categories according to what appeared to be their dominant characteristics. The categories were as follows:

The controlled type. The chief characteristic of this type was their complete mastery of the situation. They did not appear to think ahead, nor were they worried by the test in any way. They did it without any concern, hurry, or manifestation of interest.

Thinkers. This category was confined to what appeared to be abstract thinkers. They seemed to think ahead and have a definite plan in their mind before doing the test.

Good workers. This category consisted of the conscientious plodding type. However much they were baffled by the problem presented they continued to work at it. Sometimes they broke down all they

had done and started again. They were often worried by the test, but they never thought of giving it up and they learnt by experience.

The inept. This category was composed of those who had no idea whatever how to do the test. (In my original notes I called them 'fools,' and that is really the best description, for their methods seemed to be utterly irrational.)

Miscellaneous. Into this category I put those about whom I was uncertain, or who failed to impress me with any outstanding characteristic. The results obtained from the first group of subjects showed that it was better to have such a category rather than force every one into one of the positive categories.

Two further groups of subjects were then observed, using the modified technique. One group contained 259 subjects and the other 347. The two groups were entirely similar. Separate analyses of their character assessments showed no differences between the two groups, and so their results are presented together.

The industrial proficiency of the subjects was measured by a practical examination, part of which was given after one year's training and the remainder after three years' training. The examination was very conscientiously carried out, partly by those who knew the boys' work during training and partly by outside examiners. The curve of distribution of the examination marks was of the normal saddle-backed type.

Table I. *Scores in industrial proficiency examination.*

Controlled	Good workers	Thinkers	Inept	Miscellaneous
460	454	449	431	435
	455		433	

There is little difference in the marks scored by those in the first three categories, or between those in the last two, but the score of the first three is higher than that of the last two categories. In any practical application of such a method of character assessment the first three categories would certainly be accepted for employment and probably the latter two rejected. This would mean that 57 per cent. of the applicants would be accepted and 43 per cent. rejected. Dividing the subjects in this way, a biserial correlation was obtained between the character assessments and the subjects' industrial proficiency which turned out to be $0{\cdot}227 \pm 0{\cdot}037$.

It has already been pointed out that the character assessments were made while the subjects were doing the cube construction test

and, although I did not know whether the subjects would fail or succeed in the test till after the assessments were made, yet it was clear that there would be a high correlation between the two ratings, and the correlation coefficient obtained by the fourfold method turned out to be $0{\cdot}946 \pm 0{\cdot}042$.

The biserial correlation coefficient between the cube test and industrial proficiency was $0{\cdot}217 \pm 0{\cdot}037$. This correlation is of the same magnitude as that obtained by means of the character assessments, so that the same ends could have been gained by using the test alone and dispensing with the character judgments.

Before being selected for training the subjects were given a written examination, and by dividing the results of this examination in a similar way to the character assessments (*i.e.* accepting the best 57 per cent. and rejecting the rest) it was possible to work out a biserial correlation between the entrance examination and the subjects' industrial proficiency and the coefficient yielded was $0{\cdot}149 \pm 0{\cdot}038$. The same method was applied to a group of objective tests[1] (which included both the entrance examination and cube test but no character assessments) and the biserial correlation coefficient was found to be $0{\cdot}313 \pm 0{\cdot}035$.

It may be said that in a practical application of character assessments for vocational selection only those in the inept category would be rejected, the data were therefore re-examined by the methods described above but on the assumption that only 31 per cent. (the number in the inept category) would be rejected. This yielded the following biserial correlation coefficients.

Character assessments and industrial proficiency ...	$0{\cdot}209 \pm 0{\cdot}037$
Entrance examination and industrial proficiency ...	$0{\cdot}188 \pm 0{\cdot}037$
Objective psychological tests and industrial proficiency	$0{\cdot}370 \pm 0{\cdot}034$

Summary and conclusion.

The results obtained in this experiment by character assessments were better than those from the entrance examination, equal to those from the cube construction test and inferior to those from the group of objective tests suggested as a result of the investigation of which this particular experiment formed part.

[1] "A study of personal qualities in accident proneness and proficiency," By Eric Farmer and E. G. Chambers. *Industrial Health Research Board, Report* 55. H.M. Stationery Office, 1929.

An obvious criticism of this experiment or of any experiment of a similar kind is that the character categories were not well chosen or the judgments not properly made. It may be that other workers, using different categories, will get better results. In favour of the present categories it may be said that they were chosen as a result of the analysis of very full character assessments of 606 subjects, and they were suggested by observing the actual differences between the methods employed, and were not the result of preconceived psychological notions. There is some value in the work, inasmuch as fairly large numbers of subjects were observed and their character assessments compared by accurate methods with an objective criterion. Some of the work that has been done in this field ends by giving a description of the subject's character or comparing the characteristics revealed while doing a test with other characteristics of the subject known to the experimenter.

Positive results obtained by this method cannot do more than show that certain characteristics revealed while doing a test are also manifest in other situations. It cannot show how far such characteristics are helpful or otherwise in an occupation, unless they are compared with an objective measure of success in the occupation itself.

It is idle at the moment to discuss the relative value of character assessments and objective psychological tests as means of vocational prognosis. This relatively new branch of experimental psychology must be given time to develop, and there are few fields of research which offer better opportunities for the introduction of new methods in experimental technique.

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THE PHENOMENA OF ATTENTION AND VISUALIZATION IN THE PSYCHOLOGY OF MUSICAL APPRECIATION.

By P. E. VERNON.

(*From the Cambridge Psychological Laboratory.*)

I. Introduction. Method of obtaining data.

Many investigators in the psychology of musical appreciation have noted the free trains of thought and visual imagery which constitute a large part of their subjects' methods of listening to music, or, as Vernon Lee[1] aptly puts it, 'hearing' music. But there is a great deal of confusion still as to the scope and psychological nature of these phenomena. Though they and synaesthesia[2] clearly merge into one another, yet it seems to me desirable to draw definite distinctions between the three, on the basis of results obtained with a larger number of subjects of varied musical capacities than have probably been used before.

The musical responses of 104 people were investigated between 1927 and 1929, 50 of them being examined by elaborate questionnaires, given in person, and 71 attending one or more of three experimental concerts[3]. The most valuable data were obtained when conditions were partly controlled and made as similar as possible to those of an ordinary concert. I also tried employing an entirely free situation and, at another time, an unannounced programme played by hidden performers. But there are strong drawbacks to the uncontrolled and to the strictly controlled

[1] V. Lee, "Varieties of musical experience," *North Amer. Rev.* 1918, ccvii, 748–757.

[2] Cf. P. E. Vernon, "Synaesthesia in music," *Psyche*, 1930, x.

[3] My rather belated thanks are due to Dr H. Banister, Mr D. D. Arundell, and fourteen other Cambridge musicians who helped me greatly in arranging and performing at these concerts.

musical situation, which I have discussed elsewhere[1]. Thus the programme is given to members of the audience in the usual way and the following directions are mimeographed for them and explained in a preliminary talk.

After each musical item (and, if you like, during the music) you are asked to try to write down as fully as possible *what you have been thinking about while you were listening*. Mere criticisms of the music are not desired, but an account of the thoughts and feelings, etc., that you actually experienced.

First try to summarize your impressions under a few general headings such as:

A. Anything not directly related to the music, e.g. surroundings, audience, wandering of attention to extraneous matters, unnaturalness of the situation or introspection.

B. Visual images (i.e. mental pictures) of scenes, colours, patterns, etc.

C. Tendencies to action, any kind of bodily movements, response to rhythm.

D. Interest in technical aspects of the music, style, interpretation, etc.

E. The goodness or badness, or value of the music.

F. Feelings or emotions aroused in you, such as excitement, restfulness, amusement, sadness, etc.

G. The aesthetic or emotional appeal of the music to you, or the musical pleasure you obtained from it.

All of these are not likely to apply to you; if so, just use the ones that seem to you necessary to include all your thoughts and feelings while listening.

Indicate how much you attended to, thought or felt each one in each piece of music by writing down the number of the piece, then A and either 0, 1, 2 or 3 marks, where 0 means that this factor played no part in your listening, and 3 means that it played a very important part. 1 and 2 (and halves if you like) are intermediate grades. Do the same marking for the other headings, B, C, whichever you choose.

Then during the rest of the interval try to amplify these marks by describing as fully as possible the various details of what you thought about, felt or noticed while listening.

Chamber music of a high standard was used throughout, such as a Mozart Trio, classical and modern pianoforte works and violin solos, songs by Elizabethan composers, Brahms, etc.

In working up the results it was useful to be able to compare responses with the musical-ness or musical level of the subjects. This quality was determined by reliable ratings on a 0 to 10 scale[2]. The most musical subjects, University musical instructors and the like, are called Aa, Ab, etc.; the next grade, highly musical amateurs rated as having a musical level of 9, are called Ba, Bb, Bc, etc., and so on down to the least musical grade, rated 0, called La, Lb, Lc, etc. The latter were usually unmusical psychologists. Thus the first letter assigned to a subject indicates with fair accuracy how musical he was, in the following discussion.

[1] P. E. Vernon, "Non-musical factors in the appreciation of music," *The Musical Times*, Feb.–April, 1929, 123–124, 227–228, 320–321, and "Method in musical psychology," to be published shortly in *Amer. J. of Psychol.* 1930, XLI, XLII.

[2] Cf. P. E. Vernon, "A method for measuring musical taste," to be published in the *J. of App. Psychol.* 1930, XIV.

II. Physiological effects of music and wandering of attention.

I will state some general theoretical conclusions first before illustrating them and presenting the introspections and records upon which they are based.

The two phenomena, visualization and wandering of attention, may both be considered without any reference to music as an art or as involving any intellectual discrimination. They are primarily the responses of the musically uneducated, the results of allowing music to act upon them almost automatically, to sink in without their attempting to analyse it actively. Free trains of thought are, however, less evolved in the musical scale than visual imagery. They are based on the primitive, reflex physiological effects of sound, while the true visualization is essentially an emotional response, derived from what may be termed a subjective analysis of the actual music.

The stimulatory effects of noise and especially of musical noise on bodily metabolism, as investigated by Féré, Tarchanoff, Weld[1], etc., and on animals and savages are too well established to require resummarizing[2]. Music seems to increase physiological activities generally, alters and usually accelerates pulse and respiration, lowers sensory thresholds and diminishes muscular fatigue in various directions. Hence it has been put to various therapeutic uses with some success. Educated people under hypnosis, it has been asserted, regress to more primitive and overt types of musical response, the bodily expression of the emotions being very marked. Head and Holmes' thalamic patients showed similar involuntary and intensely emotional susceptibility to music[3]. Some of my subjects recalled analogous uncontrolled responses of their childhood, while even babies wave their hands spontaneously in time to rhythmic music. It is hardly rash to suppose that much of the appeal of modern dance music consists in the euphoric effects of its rhythm and tone colour, since it is seldom attended to *per se*. Except among those who, disapproving of it, inhibit it, it seems to stimulate all the muscles of the body, to produce empathically the well-known 'itching of the feet,' and so genially exercises the whole organism without the violent and overt movements which were characteristic of the older type of dances. The present popularity of Bach may well be due to much the same process. Thus, subject Bd records

[1] H. P. Weld, "An experimental study of musical enjoyment," *Amer. J. of Psychol* 1912, xxiii, 245–308.

[2] These are fully described by C. M. Diserens, *The Influence of Music on Behavior*, 1926.

[3] H. Head, *Studies in Neurology*, 1920, p. 560.

at one concert that he generally only appreciates familiar music, but "I can always enjoy Bach—a good dance—at first hearing." As is well known, singing helps sailors and primitive peoples when engaged on rhythmical tasks. Howes[1] describes some of the effects on memory and other mental and bodily processes of purely rhythmic stimulation, also the physical and moral improvements produced by folk-dancing. Presumably this is also the underlying idea of Dalcroze Eurhythmics. Among my subjects Gf noted that he can work much better the day after a concert that he has enjoyed. Most of those who answered my questionnaire, who usually attend to music actively, admitted that music in a restaurant or cinema, which is not attended to, might act as a pleasant background apart from its good or bad musical qualities. Dk added that it produces a pleasurable increase of 'general emotional tone.' Hostesses similarly exploit these stimulatory effects of music 'to get people to talk.'

Several instances of its ideoexcitatory powers may be cited. According to Delacroix[2], Darwin complained that music actually fatigued him by making him think too vigorously about his researches. The same writer quotes Stendhal: "Toute musique qui me laisse penser à la musique est médiocre pour moi." Tolstoy seems to have been similar. Bg recorded that, though he seldom allows his thought to wander while supposed to be listening, yet "it is the future rather than the past that my mind dwells upon during music." Fg says that on occasion music produces a pleasant train of thought that is clearer and quicker than usual. Hb is merely bored by bad music, attends to moderate music, and is definitely stimulated to thought by good music. Ic wrote, "Music acts as a stimulus to my mind which flutters from this thing to that."

What then is the nature of this automatic, stimulatory effect of music on the organism? It is necessary to protest against the perpetuation of early nineteenth-century physiology by some writers such as Hadow[3], who continue to state that music "directly affects the auditory nerve," usually *via* the labyrinth! To this they assign the bodily and therapeutic results.

Actually one cannot find anything much more definite to substitute for this assertion. Presumably music in some way harmonizes or regulates neuromuscular and organic processes generally, processes which are themselves periodic. By providing a background it induces a kind of

[1] F. Howes, *The Borderland of Music and Psychology*, 1926.
[2] H. Delacroix, *Psychologie de l'Art*, Essai sur l'Activité Artistique, 1927.
[3] W. H. Hadow, *Collected Essays*, 1929.

hypnotic state and seems to eliminate many of the tendencies that distract from concentrated bodily or mental activities, so lessening fatigue. These facts, it may be noted, are of considerable importance in the psychology of attention, being directly opposed to the notion of its limited, quantitative nature.

Various lines of evidence tend to show that the ideoexcitatory effects of music are more subordinated in the more musically educated. It is of course difficult to compare different subjects' ratings of their lapses, yet the following considerations prove that these ratings are fairly reliable and that they agree with general expectations. The average marks given to each item under category A (irrelevant thoughts) varied, in one concert, in exact inverse relation with the sum of the marks given to the other more musical categories. In another concert where the items were of a more homogeneous nature, the A marks varied directly with the length of each item but for one exception, a pianoforte improvization in the so-called 'ultramodern' style. This naturally produced high markings for B, C, D and F categories, and a correspondingly low A mark.

Taking now those members of the audience who wrote down 0 to 1 as their average A mark for different items, those who wrote 1 to 2 and 2 to 3, we get groups of 12 per cent., 74 per cent. and 14 per cent. who wandered little, moderately or irregularly, and most of the time, respectively. The average musical level ratings of members of these groups were 7·7, 5·7 and 2·2. Similar figures were obtained in the second concert. In my questionnaires 20 per cent. of subjects admitted that wandering of attention was pleasurable to them, while 14 per cent. stated that they always try to concentrate on the music, analysing it actively. Their average musical levels were 4·3 and 7·7 respectively. These differences indicate a large amount of overlapping, but they are fairly significant statistically, being two or more times their P.E.'s. It is obviously untrue to say that all the musical listeners attend and that all the unmusical indulge in free trains of irrelevant thought. Probably the stimulatory effects are present in all types of listeners, but are not so much a source of enjoyment in themselves among the more musical. Any musician would admit that his attention lapses if the music is very bad or boring; that personal associations with previous performances and more or less distracting thoughts of historical and technical interest tend to enter continually and may be very pleasurable. Many subjects noted that their attentiveness varied with mood, fatigue, the type of music, etc. Some are more actively analytical towards classical styles, less towards Romantic

and Impressionist. Some attend far better when they are already acquainted with the composition, others analyse new pieces and later allow them to 'sink in.'

But the nature of this analytical attention, culminating in what Sabaneev[1] calls complete 'tonal consciousness,' is outside the scope of this article. It seems that one must allow that musical people develop a capacity whereby they voluntarily repress other impressions. Eb, for example, was quite certain that when listening (with his eyes open) he did not see his surroundings at all and that he had no visual images, *i.e.* that his experiences were solely auditory and affective. Several subjects suggested that they have a musical conscience, a 'guilty feeling' when they have wandered from the music. Psychologically this conscience is probably a sentiment, in McDougall's sense. It motivates also the inhibition of the more obvious aspects of the music so that they hear the less obvious, such as the inner parts.

III. Emotional interpretations and the theory of visualization.

When specific emotions and visual images are produced, we reach a more evolved and differentiated stage than the previous physiological one. It is as if the reflex organic reactions had become qualified or interpreted, though still without any conscious objective analysis. First there is the so-called sensuous effect of timbre apart from pitch or duration relations. Thus, according to Sabaneev's experiments, there is no clear distinction among the unmusical even between pitch and intensity; a note that is either higher or louder than another note will be called clearer or sharper. Sherman[2] demonstrated that an unseen singer, by means of a single note of standard pitch and intensity, could convey emotions which corresponded well with the reports of a number of listeners. Presumably an explanation along the lines of Darwin's theory of facial expression of the emotions would hold here.

Next, by its resemblance to the sounds and motions of objects, music may suggest hurry, rest, ascent or descent, solemnity, mystery, martial tendencies, etc. One must allow that these motor responses are capable of evoking the emotion or mood which they characteristically accompany, as in Ribot's classical theory of emotional memory. Other affective states

[1] L. Sabaneev, "The musical receptivity of the man in the street," *Music and Letters*, 1928, IX, 226–239.

[2] M. Sherman, "Emotional characteristics of the singing voice," *J. of Exp. Psychol.* 1928, XI, 495–498.

are more or less permanently attached to certain types of music; sadness to the minor key, for example, is most common. In the cinematograph industry far more exaggerated uses are made of this aspect of the musical response. Cinema producers frequently use music to intensify the expression of actors' feelings in 'close ups' and emotional scenes. Writers of music to accompany films and cinema orchestra directors assume that specific pieces of music can induce specific emotions in the majority if not the whole of their audiences, though to a psychologist such an assumption seems extravagant[1]. The only confirmatory experimental evidence for it that I have found is that of Farnsworth and Bennett[2], who played music to a number of students engaged in imaginative freehand drawing. The resulting pictures were said to exhibit a considerable similarity in general atmosphere and feeling. In many concert programmes extremely fantastic emotional interpretations are read into the music even by such musicians as Cortot and Schweitzer[3], although most of the evidence goes to show that no two people appreciate in identical fashion nor spontaneously make identical interpretations. Thus Gilman[4] discovered far less agreement than he had expected among people who listened to musical items, the titles of which were unannounced but which, he had presumed, portrayed certain emotions. However, although the exact amount of community of affective responses is unknown, yet it is obvious that emotional interpretations constitute a very common mode of musical appreciation. Some sort of representational meaning is assigned even to compositions of the most abstract intentions. Composers who send in short piano pieces to publishers are usually asked to invent titles for them, though they were not originally meant to represent anything, otherwise 'they will not sell.' The type of person who is completely cold to all music except funeral marches is by no means rare; William Archer was an example. In fact many investigators have neglected practically all intellectual and aesthetic responses in favour of these uncritical emotional reactions. Gatewood[5], for instance, concludes that the more emotions a piece of music arouses, the more is the pleasure derived. Similarly Schoen

[1] For example, in Day and Hunter's catalogue of such music one finds pieces advertised to express the following: defiance, evil intentions, not guilty, torture of the soul, chastity, comic conversation and Satanic fury.

[2] Quoted by M. Schoen, *The Beautiful in Music*, 1928.

[3] A. Schweitzer, *J. S. Bach* (translated by E. Newman), 1911.

[4] B. I. Gilman, "An experimental test of musical expressiveness," *Amer. J. of Psychol.* 1892, IV, 558–576; 1893, V, 42–73.

[5] E. L. Gatewood, "An experimental study of musical enjoyment," essay in *The Effects of Music*, ed. by M. Schoen, 1927, pp. 78–120.

and Bingham[1] find the degree of enjoyment of music to be directly proportional to the intensity of 'mood effect' produced, the commonest moods being rest, sadness, joy, love, longing and reverence. Other things being equal, they say, music which produces the strongest and most diverse emotional effect is the most appreciated. Though few musicians would agree with these conclusions, yet it is clear that they hold for a very large proportion of listeners, and it is these emotional interpretations which I consider to be the basis of visualization phenomena.

The distinction which I wish to draw between ordinary wandering of attention and visualization is as follows. The former may often take the form of elaborate visual fantasies, possibly with the music or its title as a starting-point, but bearing no direct relation to it other than that due to its ideoexcitatory and hypnotic effects. But the true visualization is a kind of day-dream stimulated directly and continuously by the music as it proceeds and based either on the emotional flux or the physical responses aroused by the music. The two types of response naturally shade into one another. My theory as to the nature of the true visualization may be stated shortly and then illustrated. It is closely analogous to, if not almost identical with, the ordinary dream which, in absence of higher cortical control, represents or translates into visual terms various emotions (the latent content) and physical stimuli such as external noises, cold, indigestion, and the like.

IV. VISUALIZATION PHENOMENA.

There have been several good discussions of the phenomenon. Weld[2], Vernon Lee[3], Myers[4] and Delacroix[5] have described a visualizing type, while R. McDougall[6], as early as 1898, gave an excellent and elaborate account of his own experiences. It will be worth while quoting a few of the introspections of my best subject, Ld (though McDougall's are perhaps even better). They take the form of detailed images, scenes or dramas that are watched as if at a theatre, or rather, since the subject often takes part, as in a dream. They usually continue the whole time the

[1] M. Schoen, *The Beautiful in Music*, 1928.

[2] H. P. Weld, *op. cit.*

[3] V. Lee, *op. cit.*

[4] C. S. Myers, "Individual differences in listening to music," this *Journal*, 1922, XIII, 52–71.

[5] H. Delacroix, *op. cit.*

[6] R. McDougall, "Music imagery," *Psych. Rev.* 1898, V, 463–476.

music is going on and no other impressions seem to come into consciousness. While hearing the Mozart Trio for violin, 'cello and pianoforte, he wrote: "I am as a soul without a body floating above the sea; the sea is rough and smooth and little waves are lapping against the side of the boat, from time to time bigger waves come. They come fairly regularly. Now I am on the edge of a cliff and the waves are rolling in on the white chalk below, there is a little ledge against which the little waves in between the bigger ones babble cheerfully. Above me the gulls cry loudly, and not far away a peewit is circling and calling above his nest...."

Again, in a piano duet by Casella of an extremely cacophonous and rhythmic nature: "...There are men marching down Downing Street" (Cambridge), "they turn into Free School Lane and then down St Botolph's Lane. They are French chiefly, and there are native troops among them, they go onwards in a continuous mass, I am among them and above them marching. Now I can see the foreign legion on the edge of the desert...." A significant remark is made when listening to a later item: "I have been too much awake to appreciate this." In fact the essential state for such visualizing seems to be a kind of somnolent, hypnotic one, which some music is very effectively able to produce. Ld's introspections must have been almost in the nature of automatic writing, since they form a continuous piece of quite poetic prose, written during the music and of a different style from those written when he woke up and recorded: "My soul has returned to my body. The girl opposite has got her eyes shut and looks very foolish. How much longer are they going on for. What a lot of rot I have been thinking." In fact his only other remarks besides his visual dramas concern the performers, the audience or himself, and are almost wholly irrelevant to the music.

La is almost as interesting, and being psychologically trained writes clear introspections. Full enjoyment for him was "a dreamy state in which the instruments and motions (of the music) would seem to play on a complex emotional instrument localized in the body cavity." A piano piece by Purcell calls up, "Dancing country people in gay costumes and in bright spring landscape with flowers." The modern improvization he thought of as "A feminine piece. A woman of volcanic passions and infinite capacity for tenderness and sadness; a woman of intellectual power denouncing a faithless and small-souled lover in a sombre setting." Similar fanciful meanings are given to every item. This illustrates, also, the very close interdependence of emotional interpretations and images.

McDougall believed that his day-dreams while listening arose *via* the emotional response and could often be traced as associations with particular emotional memories or moods that were evoked by the music. Again, Myers found that the grossly unmusical, in whom the music aroused no emotion, were those who least indulged in imagery. My data are inadequate to confirm this, but it illustrates well the distinction I drew between irrelevant trains of thought and affective interpretations. There is a definite indication of a relation between emotion and imagery in the average marks given to different items for the B and F categories.

An even closer parallel appeared, however, between fatigue factors and amount of visualization. Both the average B marks and the proportion of the audience who mentioned imagery increased almost regularly throughout the evening. Naturally one cannot say how far this is due to the nature of the music, but in confirmation of the influence of fatigue we have McDougall's introspections. Also my subject Fc said that visualization only occurred with him in highly emotional music, when he was tired and carried away by the music; and now that he is more musically educated it seldom happens.

That an analytic attitude is incompatible with visualization has already been shown by Myers. In the Casella duet and the modern improvization, many of the audience gave up any attempts at analysing what they heard with the result that visualization was far more prominent. Those who tried to attend to the discords and form as such make almost no mention of images. Bg recorded that he practically never visualizes but did so in the Casella because it was technically meaningless to him. He regarded it as an attractive noise and saw fairies dancing, druidical rites and a native tribe's war dance. In answer to a question, Gf said that some composers call up "attractive visions of ideal scenes," but these disappear as he gets to know the music better and attends to it more actively. A very interesting point is that his images are much intensified by the social influence of sitting with his fiancée. Several others noted that they only visualized when the music had a definite suggestive title, words or programme (in the technical sense), and usually listen more abstractly, forgetting the interpretation, the better they know it. Delacroix gives vivid introspections of a typical visualizer who concludes: "Je n'aimais pas la musique pour elle même, mais...autant qu'elle m'avait laissée rêver." But later she received more musical training and then found that intellectual listening supplanted visualization.

The interdependent development of images and physical character

istics of music such as form, rhythm and phrasing was noted by Weld. The fantasies were not, he concluded, stories made up to fit the music, but were spontaneously and involuntarily stimulated by the unfolding of the various features of the music. Again, my subject Ek listened to a Bach Gavotte without being told the name, and saw a number of bewigged and beautifully dressed seventeenth-century people dancing gracefully in time with the music. When Mendelssohn played a Bach Suite to Goethe (a highly unmusical person, it may be noted) the latter said, "It brings before me a procession of great personages in gala dress, descending the steps of a gigantic staircase[1]." Hence also there seems to me to be a distinct onomatopoeic resemblance between my first quotation from Ld and the soothing charm of the Mozart, and between the second and the dynamic dissonances of the Casella. It is interesting to observe too how the unusual features of the latter are rationalized, as in typical dream translation, by appearing as foreign soldiers. Actually one-fifth of the audience employed similar symbolizations of the barbaric and martial qualities of the music, visualizing native war dances, wooden or foreign soldiers and the like, although the title of the piece as announced was "Polka."

With regard to suggesting specific scenes, Gilman, Weld and Downey[2] obtained negative results, except when listeners were told the title of the piece. The latter actually evoked vivid images among her subjects of hunting when playing a piece meant to represent "In a Clock Shop," which was mistakenly announced as "A Hunting Scene." Similarly Wells[3], playing ten unknown gramophone records to an average of nineteen people, asked them to identify which of the ten names belonged to which. On an average only six people identified the name of any one piece correctly, but seventeen were right in a piece called "Longing for Home," and nine in the Siegfried Funeral March. Many of the bad errors of identification were also fairly obvious; for example, nine subjects mistook the "Ride of the Valkyries" for the "Devils' March." He concludes that music tends to suggest very different images to different listeners, even from those intended by the composers.

A summary list of the images aroused by the modern improvization is interesting and shows the degree of consistency possible, when no kind of title nor representational meaning is given beforehand.

[1] Quoted by A. Schweitzer, *op. cit.*

[2] J. E. Downey and G. E. Knapp, "The effect on a musical programme of familiarity and of sequence of selections," essay in *The Effects of Music*, ed. by M. Schoen, 1927.

[3] F. L. Wells, "Musical symbolism," *J. of Abn. Soc. Psychol.* 1929, XXIV, 74–76.

Bf: Fountains (by auditory association with the style of Ravel's "Jeu d'Eaux").
Df: Chord blocks and vague colours (really a synaesthetic rather than a visualization phenomenon).
Di: (Reminded of) a child in a bad temper, no control over its feelings.
Dk: Puck going round the earth in forty minutes (by auditory association with Debussy's Preludes).

Thus there seems to be no true visualization above the musical level of 6.

Ef: Chaos, earthquakes, other worlds fighting, then a quiet spirit.
Eg: Busy London thoroughfare, traffic.
Fd: Utterly meaningless blue-green shapes running into one another suddenly, hit by strong reds (probably synaesthetic).
Fi: (Reminded of) an iron foundry.
Gb: Representation of chaos (no details).
Hc: Puff, puff of an engine starting.
Ii: A madman having fits, general violence, wildly thumping hands, etc.
Kd: Sea breaking on rocks.
Ki: General ideas behind futurism.
La: A woman of volcanic passions, etc. (see above).

One might be tempted to regard visualization as the response of those whose auditory imagery is so poor that they tend to interpret what they hear in emotional and visual terms; Dalacroix suggests this. From my results I should say that it is only true in the limited sense that the better musicians who usually have good auditory imagery also do not often visualize while listening. But there were plenty of cases of well-trained musicians who never visualized but who also possessed no true auditory imagery. Nor is the response necessarily a characteristic of those who have good visual imagery in ordinary life, any more than are night dreams. Like McDougall, several visualizers had poor visual imagery normally, and among questionnaire subjects I found it impossible to establish any significant relation of this kind.

There remains the matter of its distribution and frequency. Most writers have considered visualizers as a distinct and common type, though Myers allowed that almost everyone visualized more or less often. I find that ordinary visual associations, which can hardly be distinguished from mere wandering of attention, occur in the majority of subjects. My concert audiences were a rather highly selected group, yet half of them mention imagery at some time. But the true visualizer, who alone can be called typical, is decidedly rare. His two main characteristics are, first, that his imagery, like that of McDougall, follows the music and is based on it throughout (what I called above subjective analysis). Secondly, the imagery is enjoyed for its own sake and constitutes, in fact, almost his sole source of pleasure. Again, of course, intermediate cases are commoner, *i.e.* subjects who respond in this way for short periods. So that no strict dividing line can be drawn between attention lapses, trains of

irrelevant and relevant associations and fantasies and the extreme type of day-dreams. At two concerts not more than 10 per cent. seemed to enjoy the imagery *per se* at all frequently, while scarcely 5 per cent. depended on it for their main method of appreciation. More adequate introspections might easily have elicited more, for, like Myers' subject M, there were some people who supposed themselves to listen wholly abstractly but who, in the actual concert situations, produced some quite elaborate imagery. Again, my subjects were all highly educated, intelligent people; one might expect more of this passive day-dreaming among those who are not University graduates or undergraduates.

It is worth noting that no marked sex differences appeared.

Vernon Lee divides her 200 odd subjects into musical 'listeners' and unmusical 'hearers,' saying that the latter always weave some kind of emotional or visual drama into the music, appreciating it because they find "a meaning or a message in music, something beyond itself." At my concerts the average musical level of those who mentioned imagery was 3·6, while of my questionnaire subjects one-third, of musical level 7·2, assured me that they never visualized at all. I have no intention of discussing here the rôle of emotion in aesthetic appreciation, but there is certainly a tendency for the musician to regard the appearance of concrete emotions and interpretations as irrelevant or as a sign of the music being definitely bad, though there are many exceptions (such as Cortot and Schweitzer, as already mentioned, and Howes[1]). From questionnaire answers I found it possible to classify 19 per cent. and 28 per cent. into Lee's 'hearers' and 'listeners,' with some distortion. One example of each class may be quoted. Ka says, "I much prefer music of love and sadness as it gives me such a restful feeling and quite takes one out of oneself." Hence "I am faithful to Wagner as there is something in all his music which is so majestic and so true to life." Ad says that he can hardly recall a single instance of obtaining a distinct emotion from music, he gets "a purely musical, intellectual pleasure, a unique mystic experience." The average musical level of the two groups is 4·4 and 7·5. But the majority, ranging over practically the whole scale of musical levels, fall into the intermediate class. Some of the fairly musical, for example Be, Dd and Ga, said that they obtained 'a state of mind,' 'a general impression' and 'an atmosphere' rather than specific pictures. Probably, apart from a few typical visualizers and a few highly trained musicians, the average listener indulges in short trains of association and lapses of attention, and while he frequently makes emotional interpreta-

[1] F. Howes, *op. cit.*

tions, particularly of music to which words or a title are already appended, yet he also often listens technically and analytically, according to mood, familiarity, type of music, etc. He combines, more or less, all types of appreciation. Thus no hard and fast classification like Lee's can be upheld, though there is undoubtedly a tendency in this direction.

Finally, a slight correction in the Myers and Valentine classification is necessary as regards normal musical appreciation. The most typical visualizer seems to belong to a sub-class of the intrasubjective or emotional type as much as to the association or attention wandering type[1].

V. SUMMARY.

1. The musical appreciation of a large number of subjects was studied by means of questionnaires and specially devised concerts, where written qualitative and quantitative introspections were obtained.

2. Music has striking physiological, almost automatic, effects on the organism, stimulating bodily and mental activities. Wandering of attention and free trains of thought are often excited and may be highly pleasurable. The more musical, owing to a sentiment or musical conscience, tend to inhibit these primitive responses and to attend more actively to the music.

3. The resemblance of music to the sounds and motions of physical objects, especially when supplemented by suggestive titles or words, may arouse a flux of concrete emotions on the basis of which listeners build up elaborate dramatic interpretations or visual dramas, closely analogous to ordinary dreams. These are enjoyed for their own sake.

4. Though in most subjects lapses of attention may lead to trains of imagination, this imagery, which is directly stimulated by and which follows closely the form, rhythm, etc. of the music, is comparatively rare. It is incompatible with an objective attitude towards music, and is almost wholly absent among the most musical. Illustrations are given of these points, of the relation between visualization and emotion and fatigue, and of the degree of agreement between the interpretations of different subjects.

[1] For further discussion of the relation of Myers' character type to the intrasubjective and associative types, see P. E. Vernon, "Synaesthesia in music," *Psyche*, 1930, x.

(*Manuscript received* 10 *January*, 1930.)

AN APPARATUS FOR THE PHOTOGRAPHIC RECORDING OF EYE MOVEMENTS.

By M. D. VERNON.

(*From the Cambridge Psychological Laboratory.*)

The apparatus described in this paper is based upon a modification of Dodge's method of recording eye movements photographically[1]. By means of the former, a record is obtained upon a continuously moving film of the movements of a bright spot of light reflected from the cornea. It has been shown by Dodge[2] that the movements of such a spot are the same in direction as those of the eyeball itself, but are only about half as great in extent. Fig. 1 shows the apparatus viewed from the front, and Fig. 2 shows it in section along a vertical plane, parallel to the sagittal plane, passing through the subject's right eye.

The source of light is a 100 c.p. Ediswan Pointolite bulb; the light is emitted from a small very bright ball, which forms an excellent point source. The light from the bulb D (Fig. 1), contained in the brass chamber C, passes through a blue gelatin filter and the narrow aperture, 3 mm. in diameter, of a diaphragm placed at J. The divergent beam of blue light passes along the brass tube T_1, impinges on the right-angled prism P_1, and is totally reflected on to the cornea of the subject's right eye, as shown in Fig. 2. The beam is reflected by the cornea in a direction parallel to the sagittal plane, at an angle of about 50° with the horizontal; and impinges on a second right-angled prism P_2, by which it is refracted and totally reflected into the brass tube T_2 of the camera M. The divergent beam is focussed by the lens L, a Ross, London, Xpres camera lens of aperture 1 : 4·5 and focal length $4\frac{3}{8}$ in., and after passing through the tube T_2 and the aperture of the camera at A, forms an image on the film at F_1. In Fig. 2 the camera is shown in position for a vertically moving film. The latter passes from the film spool F_2 over the two pulleys O_1 and O_2, and is attached to the heavy brass pulley O_3. The drive is conveyed from a motor and reducing gear to the fourth pulley O_4, the shaft of which engages with the shaft of O_2 by means of a worm gear. Thus the film travels smoothly downwards past the aperture A, and is wound on to the pulley O_3. In these experiments it was driven at a

[1] Dodge, R. and Cline, T. S., *Psychol. Rev.* 1901, VIII, 145.

[2] Dodge, R., *Psychol. Rev. Monog.* 1907, VIII, No. 4.

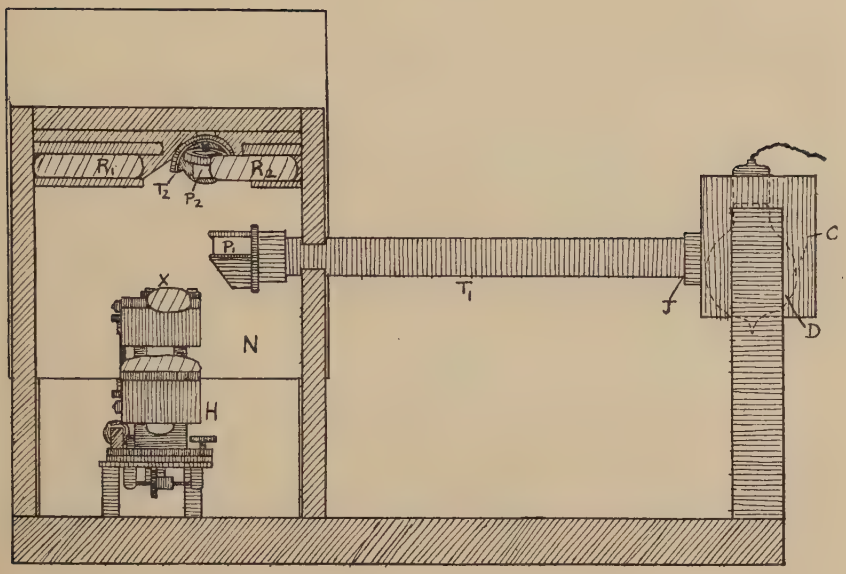

R1
T2
P2
R2
P1
X
N
H
T1
f
C
V
D

Fig I

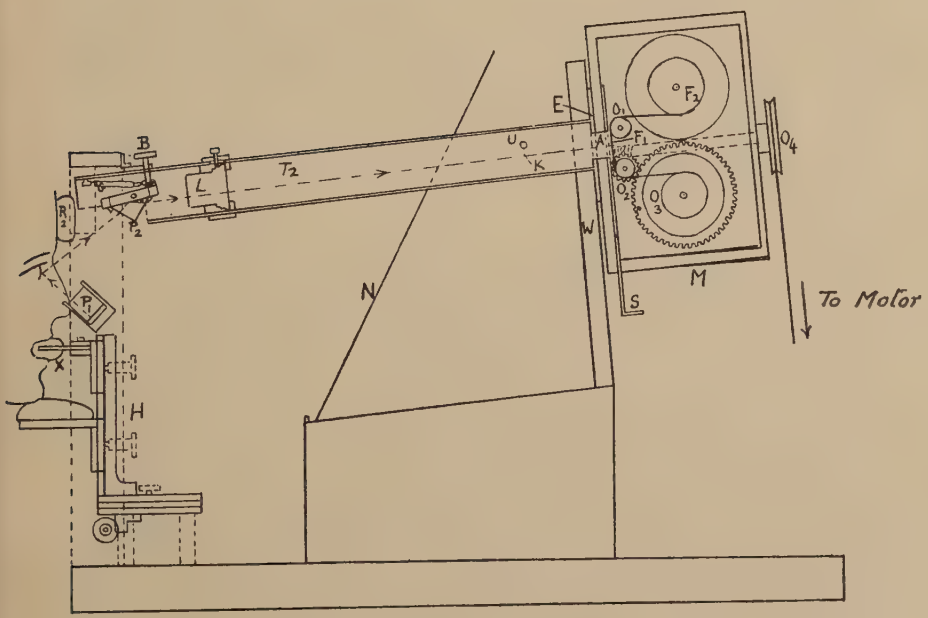

E
O1
F2
F1
O4
B
T2
L
U0
K
W
O2
O3
M
S
To Motor
N
R2
P1
X
H

Fig II

speed of approximately 0·5 cm. per second. The aperture can be closed by means of a shutter, S, which slides in and out.

The experimental procedure is as follows. The subject places his head in position on the head-rest, H, with his chin on the chin-rest and his forehead pressed against the two adjustable pads R_1 and R_2. He grips with his teeth a piece of dentist's wax X; an impression of his teeth is taken, so that they can be held firmly in the same position throughout the experiments. Both the mouth and chin-rests can be adjusted vertically; and the head-rest can be moved as a whole in two horizontal directions at right angles to each other. By turning the tube T_1 about its axis, the beam of light is directed to form a bright spot of light on the cornea. The angle at which it falls on the cornea is such that, when the eye looks straight forwards, the beam does not enter the pupil, and is thus not visible to the subject. The reflected beam is directed along the tube T_2 by rotating the prism P_2 about its horizontal axis. The rotation is effected by raising or lowering the screw-head B; the prism is kept tightly pressed against the screw by means of the spring G. If necessary, the position of the head is also adjusted. The camera is previously removed from the upright W, and a piece of ground glass at the end of a tube placed in the position normally occupied by the film; the reflected image is then focussed on the ground glass by sliding the lens L towards or away from the camera. The latter is then replaced, the motor started and the shutter opened. A photographic record is thus obtained of the movements of the eyeball, made either voluntarily or in reading. The reading material, or a sheet of paper bearing a series of points to be fixated, is placed in front of the subject against the screen N.

The camera is held steadily in position by flanges fixed to a metal plate E, which in turn is screwed to the wooden upright W. If the screws are removed, the camera and plate can be swivelled about the tube T_2 until the camera and film are horizontal, and the metal plate again screwed to W. With a vertically moving film, a record of the horizontal components of the eye movements is obtained, and with a horizontally moving film, a record of the vertical components.

A time signal is used for measuring the exact speed of movement of the film. A narrow convergent beam of light is directed through the aperture U on to a small piece of plane glass K situated on the right-hand side (facing the subject) of the tube T_2. The light is reflected by this piece of glass to form an image at the right-hand edge of the vertically moving film. A time marker (not shown) placed outside T_2 in the path

of the beam allows the light to flash once a second on to the film, thus giving a time record which does not interfere with the eye-movement record. This time signal can only be utilized with the vertically moving film.

This apparatus has been found to work quite satisfactorily, and a number of records have been obtained both of eye movements made voluntarily and of those made in normal reading. Fixations appear as fine black lines on the film. The speed of movement of the film is sufficient to show clearly the fixations and re-fixations which occur in normal reading. Voluntary movements up to 40° of arc can be recorded; they cover about 1 cm. on the film. By use of magnification, movements of 1° of arc and less are readily measurable.

In conclusion I wish to express my gratitude to Prof. Hartridge for his advice and help in designing this apparatus, and to Dr E. A. Schuster for constructing it at the National Institute of Medical Research, Hampstead; and to thank the Medical Research Council for permission to use the accompanying figures.

(*Manuscript received* 20 *December*, 1929.)

A NEW TYPE OF SPHYGMOGRAPH.

By W. A. WILLEMSE.

A. Difficulties of existing sphygmographs.

All existing types of sphygmograph present considerable difficulties as follows:

(*a*) In sphygmographs with a wrist attachment the mode of transmission of the pulse-beat through a button held in position on the artery by a spring is inefficient. At the place of attachment the ordinary wrist is a bundle of tendons and bones, which often makes it difficult to get the lever button exactly on the artery and deep enough to register satisfactorily. Furthermore, the wrist is very pliable and responsive to slight finger movements, which may make the results registered difficult to interpret accurately.

It is, in fact, far better to make use of the brachial rather than the radial artery. On the inside of the elbow the brachial artery is very conveniently exposed. The muscles are larger, and do not cover the artery at all. The fore and upper arm can be conveniently fixed in the apparatus constructed for this purpose, and with the necessary precautions all uncontrolled movements can be eliminated (see Fig. 1 *a* and *b*). That the possibility of uncontrolled movement must be eliminated before the sphygmograph is attached is a principle disregarded by all the ordinary types of sphygmograph.

(*b*) Sphygmographs like those of von Frey, Jaquet, Frank-Peter and Marey are not sufficiently sensitive. Every one of these is some modification of the original principle of the Marey sphygmograph (Fig. 2), in which a fairly strong spring is used to press the button on the artery. Much of the energy of the pulse drive is absorbed in overcoming this spring and is not available in order to displace a larger volume of air for transmission purposes to the Marey tambour. This difficulty is to some extent overcome in Lehmann's instrument, but this sphygmograph cannot be properly fixed on the pulsating area. However, if modified in this respect—as in the type I suggest in this article—the Lehmann sphygmograph is very sensitive and is simple to work.

(*c*) Sphygmographs which do not make use of an air transmission can produce curves of about 1 cm. in height, but the chronographs attached to them make such a humming sound, and vibrate on the arm

so perceptibly to the subject, that no accurate experiment is possible. Moreover, the spring which drives the mechanism cannot, without rewinding, go for a sufficiently long period to make even a moderately lengthy experiment possible. Also it is hardly possible to attach to these sphygmographs a time recorder, *e.g.* a Jaquet writer, and a stimulus recorder.

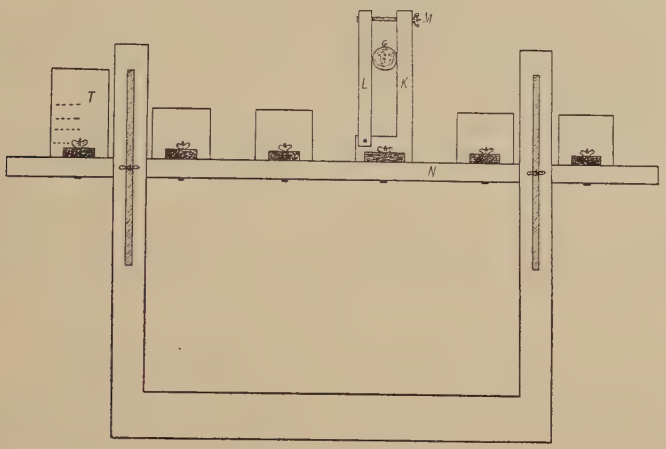


Fig. 1 *a*. Side elevation.

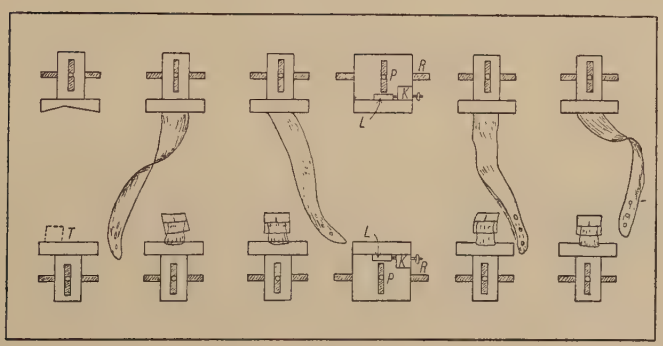


Fig. 1 *b*. Top elevation.

(*d*) The brachialis pulse-wave writer, though it escapes the pulse difficulties, does not seem to be applied to a sufficiently exposed small spot for sensitive recording of the pulse-beat. To get the Manschette firmly upon the deeply lying brachial artery such a pressure is necessary that the finer pulse changes are obliterated.

B. Proposed new form of sphygmograph.

The type of sphygmograph I propose to some extent overcomes almost all these difficulties. In details it may still be much improved, and a very important central improvement would be to find a means of eliminating the air transmission which always to some degree entails a source of error. This could perhaps be done by having the sphygmograph fixed to a rigid frame, and adjusting the arm to the pelotte instead of adjusting the pelotte to the arm. Since in the sphygmograph in question the application is at the brachial (inside of the elbow), some such arrangement seems to be possible. In that case an ordinary kymograph, equipped with a Hering's extension, could be comfortably used.

The main idea of my modification of the usual sphygmograph will be quickly grasped by looking at Fig. 3. The air funnel *W* is covered with

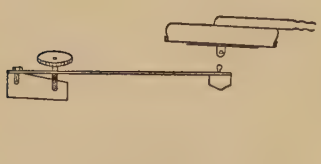


Fig. 2.

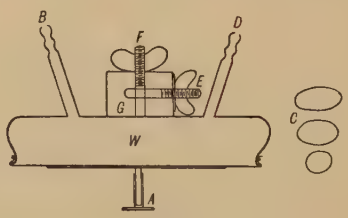


Fig. 3.

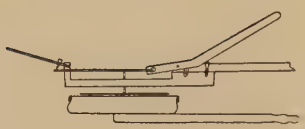


Fig. 4.

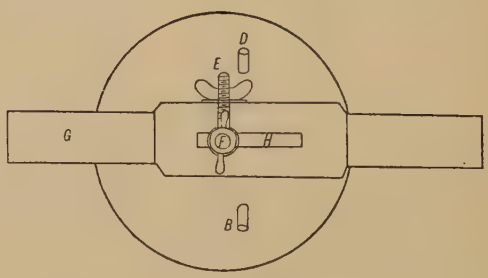


Fig. 5.

an india-rubber membrane to which a fairly wide aluminium circular plate is attached. *A* is a detachable pelotte, the form of whose base may be varied (see *C*) to fit unusual types of fatness accumulation on the arm. To the one jet, *B*, a tube connected with a Marey tambour is attached. This Marey tambour (at the kymograph) is provided with a double lever to augment the size of the curve drawn. Now considerable force is necessary to move the double lever (Fig. 4), and this other

sphygmographs are unable to provide, because much of the pulsation energy is lost in overcoming the spring, and also because the volume of air displaced in the tambour at the pulsation spot in none of the other sphygmographs is sufficiently large to force the double lever. The other jet, *D* (Fig. 3), is connected to a small india-rubber squeezing pump and, if experimental aims require it, with a manometer as well. For registering the pulsation a very weak inflation is required and no pump or manometer is necessary ordinarily. If use is made of a pump, etc., the tube must be securely closed off when the correct pressure is attained, because too large a volume of air provides an elasticity which absorbs the pulsation thrusts to some extent. The air-funnel is attached to the

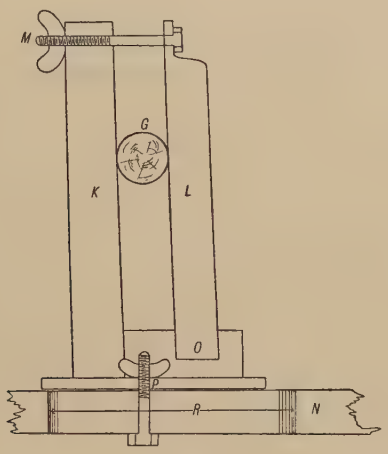


Fig. 6.

piece of wood *G* (Fig. 5), with the bolt *F*, which can be moved up and down, as well as to and fro, in the slit *H* in the wood. When the best position has been attained it is fixed by the bolt *E* and, if necessary, the wing nut at *F* can be tightened as well. The piece of wood *G* is round at the ends, in order to be able to turn between the vertical calliper jaws *K* and *L* in Fig. 6, before the nut at *M* is screwed on. In this figure *N* represents the flat board on which the arm is fastened, as is completely shown in Fig. 1. The slit *R* in the wooden board *N* (Fig. 6) enables the operator to adjust for different arm-lengths, while the slit at *P* in the metal stand makes adjustment according to arm-breadths possible. It is imperative, when fixing an arm to the apparatus, that the position be a comfortable one, without interfering with the optimum position of the exposed artery spot. I found the best position to be that where the

wrist and hand lie on edge, while the inside of the elbow points nearly straight in an upward direction. Wadding or cloth should be placed between the arm and the fixtures. Finger movements caused muscle-shiftings, which affect a sensitive apparatus of this type to some degree. For this reason the hand is clamped at *T* in Fig. 1 with slits of iron between the fingers, as indicated by dotted lines.

With this apparatus I was able to obtain curves 5 cm. high, whereas with the old method the curves were from $\frac{1}{2}$ mm. to 5 mm. at best. The form of the curves is also very characteristic, individually different and distinct.

(*Manuscript received* 3 *November*, 1929.)

CRITICAL NOTICE.

GESTALT PSYCHOLOGY AND GESTALT THEORY.

By O. A. OESER.

(*From the Psychological Laboratory, Cambridge.*)

From time to time the attempt is made to put a science or a philosophy on an entirely new basis, to set up all-embracing, all-explaining theories on which future research has to be based. Any such system must naturally concern itself with a critical examination of the foundations of that science or philosophy. It will have to justify itself by constructive criticism of existing systems, and will naturally come into conflict with existing ideas and prejudices. It must, therefore, expect to have its own postulates and deductions critically examined. Moreover, the sharper its own tone, the more intolerant of existing interpretations it is, the stronger will be the attacks levelled against it.

These remarks must serve as an excuse for the controversial tone of this paper, which will try to review some of the theories set forth in Dr Wolfgang Köhler's latest work[1]. It is claimed that nothing so fundamentally new has appeared since the time of William James and that this book will revolutionize every aspect of Psychology. The whole tone of the book is extremely controversial or dogmatic; certain statements are made over and over again, as though mere reiteration could *prove* anything. It has not been an easy task to sift and select; but wherever it was necessary, we have quoted Köhler's own words to prevent misunderstanding as far as possible.

Like all other philosophies and most psychologies, *Gestalt* psychology asks, what is the nature of the higher mental processes and what part do they play among the phenomena of the universe? The answer to this question is a complete inversion of all idealistic systems and an attempt at a thoroughgoing materialism. What we call 'mental' structures, are supposed to be entirely based on the physical structure of nervous processes. Ideas rest on physical reality and not on any platonic '*eidos*' or *a priori* mental categories. This is thought to be possible because within the physical world we find structures to be operative that are in all respects similar to the structures of the higher mental life, namely,

[1] Dr Wolfgang Köhler, *Gestalt Psychology*, G. Bell & Sons, London, 1930. The page references in this notice refer to the original American edition.

physical *Gestalten*. These physical *Gestalten* are the ultimate foundations of mental life; and the formal structure of mental life is already present in physical processes. These foundations can therefore not be arrived at by the method of introspection, since the attitude of introspection changes an experience during and by the process of introspection; nor by the purely arbitrary and external methods that behaviourism imitates from physics, since these are based on a mistaken idea as to the functional validity of atomistic mechanical laws. *Gestalt* psychology shows that such laws do not exist for the mind: it thinks that the *real* laws are known to us immediately in 'direct experience' and that we need only the naïve acceptance of 'direct experience,' and no other principle than that of 'dynamical self-distribution,' in order to explain the structure of the mind[1].

This applies to every form of psychological experience: "According to the theory of dynamical self-distribution color as a quality is dependent upon chemical reactions in a brain-field.... There is much more that is characteristically spatial and dynamically real in such a coherent distribution than in the mosaic of local sensation-processes which are the 'dynamical realities' of machine theory[2]." Why do we experience 'constancy of brightness'? What is an attitude, an instinct, attention, admiration, the quality of 'softness' or 'roundness' in a voice and why do we experience them? Because they are based on dynamic tensions and stresses in dynamic physiological *Gestalten*, because, or just as, potential difference, charge distribution, surface tension, orbital acceleration, are physical *Gestalten*.

What is a *Gestalt* and whence does this word derive its potency? It seems to be largely a case of word-magic. Let us take some of Köhler's definitions: "In German the word 'Gestalt' may be used as a synonym for 'form,' or perhaps 'shape.' So von Ehrenfels, taking the case of specific shape as the most important and evident among his qualities, applied the name of 'Gestaltqualitäten' to all of them. Therefore it will be clear that not only the different forms or shapes of objects and figures are included, but also qualities like 'regular'[3]." Spatial and temporal 'Gestaltqualitäten' must also be included. But since the time of Goethe *Gestalt* connotes shape and form as a *property* of things, as well as "a concrete individual and characteristic entity, existing as something detached and *having* a shape or form as one of its attributes[4]." But it is chiefly in German literature of to-day that the word *Gestalt* figures and is clothed with a mystical meaning. As it is used to-day, the word means the *ideal* configuration, which shines through its earthly manifestations. It is important to remember this, for it seems to have led large groups of people into believing that this aspect of the word had at last found an incontrovertible scientific basis in the proof

[1] See below, pp. 76 ff.
[2] P. 190.
[3] P. 191.
[4] P. 192.

that there are physical *Gestalten*. Since the theory clothed itself in a veritable wilderness of differential equations, and never ceased emphasising that *Gestalt* was the solution of every fundamental problem in physics or psychology, it was accepted without further comment by those incompetent to judge, or ignorant of physics, and hailed as a revolutionary theory[1]. But let us return to Köhler. "Gestalt means any segregated whole." "The same general type of dynamical process which leads to the formation and segregation of extended wholes *will also explain their specific properties*....This, indeed, is the most general concept of *Gestalttheorie*: wherever a process dynamically distributes and regulates itself, determined by the actual situation in a whole field, this process is said to follow principles of *Gestalttheorie*....In consequence the concept of *Gestalt* may be applied far beyond the limits of sensory fields[2]." In general, "*Gestalten* are states and processes whose specific properties and effects cannot be compounded from properties and effects of a like order shown by their so-called parts[3]."

These definitions are so wide that they tell us nothing. A *Gestalt* is any segregated whole; it is also a state or a process; it is more than the sum of its parts. A chair is a whole; it is more than the sum of its parts, since I cannot sit on any of its parts. Is a chair a *Gestalt*? However, for reasons of word-magic we must retain this word, the more so since Köhler believes that it also stands for a new concept in physics.

We shall have occasion to return to these definitions later. Let us first follow Köhler in his exposition of the principles that underlie psychological events and the reasons for believing them to be in all respects similar to certain events in the physical world. We need not concern ourselves very much with the first chapters, which deal with the viewpoints of Behaviourism and Introspectionism. He presents these theories in an extraordinarily narrow manner. No psychologist of any standing has ever held them in this form. He frequently digs up old doctrines that have long since been discarded. Naturally, it is easy enough to smash up any theory, if only one presents it in a sufficiently one-sided fashion. If Introspectionism really is the simple and relatively crude theory it is made out to be, we should have thought it unnecessary for Köhler to have taken such pains to demolish it.

Instead of using the word 'consciousness,' which to some people means a function by or in which we become aware of 'immediate experience,' Köhler proposes to use 'direct experience' throughout, since, if someone has a 'feeling of becoming aware,' that is merely one special case of direct experience. For a child—or a layman, whose judgment on many fundamental aspects of psychology is far more to be trusted than

[1] Cf. the critique by Jaensch and Grünhut, *Über Gestaltpsychologie und Gestalttheorie*, Langensalza, 1929.

[2] P. 193. Author's italics.

[3] W. Köhler, *Die physischen Gestalten in Ruhe und im stationären Zustand*, p. xiv, Braunschweig, 1920.

that of trained introspectionists of philosophers (cf. chapter on 'Insight') —objective experience is not something "felt by me as being 'given in my perception' and therefore as 'subjective'." Things and events are *simply there outside*. Anything more objective is inconceivable to the child, since they are independent of its presence, or of its keeping its eyes open. Therefore, in this book, when Köhler speaks about 'direct experience of the objective type' he means "e.g. a chair as something there outside, hard, stable, brown, generally without any taint of 'being perceived' by me, of being 'a subjective phenomenon'[1]." Now for the behaviourist there is no such thing as direct experience (*i.e.* consciousness); for the introspectionist direct experience changes in the process of being observed. Furthermore, physical measurements are indirect, and the physical world, that is built up by inference and is known only mediately is, in a sense, subjective. No two physicists can ever observe 'the same galvanometer,' since each only has his direct experience to go upon. But, Köhler argues, extreme epistemological purism shows that the existence of the physical world can never be proved conclusively, nor the existence of direct experience in others. "What is needed, then, is a little less epistemological purism and a little more *naïve* pragmatism." The starting-point for psychology must be that 'direct experience of the objective type' which the child has. The necessity for this becomes even more cogent when we remember that every science started off with purely qualitative observations and that psychology, being only in its infancy, cannot imitate the most advanced methods of physics or physiology, being, for one thing, unable to create its own systems and regulate their controlling factors. Psychology should at this stage confine itself above all to qualitative observations.

But instead of adhering to this principle, and giving us his qualitative observations, Köhler goes on to suggest a new basis for building up a working hypothesis and formulates the following simple principles[2]:

(1) *Naïve description.*

We must always utilize the direct experience of the subject and accept it at its face value, since it will always give us hints about the psychological processes we are seeking.

[1] Pp. 21–22. The sentence is presumably meant to read: "a chair [experienced] as, etc." Direct experience is thus simple awareness, untainted by introspection, meaning or experience, or reason.

[2] Köhler does not classify or name them as here given. That has been done by the author to make them easier to refer to.

(2) *First primary psychological postulate.*

"There is no doubt that this experience depends upon some of those processes about which we wish to formulate a working hypothesis. It is more than probable that, under these circumstances, I may use my direct experience in order to guess about these processes[1]."

(3) *The psychological theorem.*

"There is no reason at all why the construction of physiological processes directly underlying experience should be impossible, if experience allows us the construction of a physical world outside, which is related to it much less intimately[2]."

Thus, if the layman feels 'admiration' for someone or something, he also feels this as a 'directed tendency' in direct experience. Therefore, not only is there a physiological process underlying the 'admiration,' but in it, or between it and the process underlying the self, there will be a dynamical stress, or a vector (cf. chapter on Insight). The most astounding part of this theorem is that it should be just as possible to construct this physiological world on which our experience is based, as it is to construct the physical world—which we know only through the mediation of physiological processes! Is there one of these dynamical physiological processes that we can isolate and study? Only 'direct experience' tells us about them, and to everyone except Köhler this is a very uncertain witness. The physicist has long ago discarded it in its naïve form. He does not assume that a dish of lukewarm water is hot because his hand, which has just been immersed in ice, gives him the experience of hotness[3].

(4) *Second primary psychophysical postulate.*

In order to guarantee the correctness of my inferences from direct experience to the underlying physiological processes I must assume *congruence or isomorphism in their systematic properties*[4].

(5) *First secondary postulate.*

The physiological processes are *dynamical* units.

(6) *Second secondary postulate.*

The physiological processes corresponding to different parts of an experienced visual field in which there are a number of configurations are kept from intermingling by '*separating forces of contact*' (see below, p. 82).

(7) *Inferences from* (1)—(5).

(*a*) *Order.* We are led at once to "a bold hypothesis which has far-reaching consequences, if it is taken seriously enough." If we find

[1] Pp. 59–60.

[2] Pp. 60–61. This passage is italicized throughout.

[3] See the discussion of physical principles below, and particularly the summary at the end of this paper, pp. 93 f.

[4] P. 61.

in direct experience that one point, whether spatial or temporal, lies between two others, this experience must be accompanied by "a functional 'between' in the concrete dynamical context of concurrent physiological events," simply because it is an immediate experience. I do not reason about the matter: the 'betweenness' is itself an experience and must necessarily have its corresponding physiological process. Hence "*experienced order in space (or time) is a true representation of the corresponding concrete order in the underlying dynamical context*[1]."

(*b*) *Language as a symbol.* I write down or speak about my observations, and the language is taken by me to refer to my own direct experience, which has a concurrent physiological process. "Therefore, if, to me, my language is an adequate symbol for my own direct experience, it is an objective symbol for those physiological processes at the same time[2]."

(*c*) *Context.* If I sit in a concert hall, I hear several singers, my neighbour's snores, the shuffling of feet and numerous other sounds. I also have a large number of visual experiences. Nevertheless, they are not all inextricably mixed; the sounds coming from the singers are experienced as 'belonging together,' those from the accompanying orchestra 'belong together,' and so on. This 'togetherness,' the *context*, in this example as in that of a sentence or any other case, is a direct experience. Therefore, "*To a context, experienced as 'one thing' belonging together, there corresponds a dynamical unit or whole in the underlying physiological process*[2]."

(8) *First axiom of originality.*

"This last application of the principle (7 *c*) has the greatest importance for *Gestalt* psychology. In this form, at least, it becomes a radical physiological hypothesis about common sensory experiences *as well as about the most subtle and complicated processes producing observable behavior*[3]."

(9) *Second axiom of originality.*

Dynamics itself *produces* the order found in physiological events[4].

Instead of 'a little more naïve pragmatism' Köhler has given us a most formidable list of assumptions, most of which seem to assume in advance what he is really setting out to prove. And yet, how simple it all sounds. I see a figure composed of five dots, ⁘, as a whole: the under-

[1] Pp. 64–65. Köhler's italics.

[2] Pp. 66–67. Köhler's italics.

[3] P. 66. Author's italics.

[4] If it is not an axiom, it is mere tautology for the law of action and reaction in physics, which is discussed later. It is original, because no one ever thought of dynamics having this function before.

lying physiological process must be a unit. I have a direct experience of joy, feel anger, hear a symphony, admire a singer, speak a sentence, experience the sum of my spoken sentences as a good speech, see that one tree stands between two others, feel charmed on perceiving the first smile of my child[1]: behind all these experiences there are physiological dynamical units, or dynamical stresses. What sort of dynamics? Where do these dynamical processes take place? What is it that is dynamically interacting? But we must not ask questions like that, because, "Confident as we are of further help from physiology, we are so utterly in the dark at present that it would be pure folly not to profit by any light that may fall upon some cases at least.[2]" And at any rate we shall obtain some light from a study of physical dynamic systems.

So far, we do not seem to have got anywhere in particular. We have, it is true, some "bold and radical physiological hypotheses with far-reaching consequences." The hypothesis that to every experience whatsoever there corresponds some physiological process is not new. But the *formal* structure of experience, its systematic properties, are also supposed to be found in these processes. Since in 'direct experience' no such thing can be found as 'a *mosaic* of indifferent stimuli,' therefore any such physiological process is a *dynamical unit*. Since my experiences show a certain spatial or temporal order, this order must be due to a self-regulating property inherent in all dynamical processes, and cannot be due to mechanical arrangement. Out of the material processes spring supra material factors and it is these that make experience possible. How does this come about? We must study the world of physics to get an answer.

We find in nature, Köhler says, two factors: constant topographical *conditions* and dynamical *processes*. The constant conditions are the goalers of the dynamical processes, since the more 'constant' factors we introduce, the more restricted is the play of dynamical forces. Thus, in the internal arrangement of an atom the dynamical factors are allowed greatest latitude, less in the distribution of charges over a conductor, still less in the piston of a machine, where only *one* direction is possible. Now—and this is the second axiom of originality (cf. p. 78)—it has always been thought that the only *order* that is ever to be found in nature is that which is enforced by topographical conditions (as in industrial machines). If dynamical forces are allowed free play, only chaos would be produced. But this view is quite mistaken[3]. If we study the atom, or electrically dissociated solutions, or a drop of oil in water, or con-

[1] P. 353. [2] P. 60. [3] That is why it is *not* held in physics!

densers, or any other physical system, we see there a beautifully harmonic, orderly distribution, which is brought about by the free dynamical interaction of various forces. "Everywhere in nature dynamical events depend upon the properties of those processes and materials which exert influences upon one another[1]." But hitherto it has been assumed that only machines were orderly, and nerve currents were therefore thought to be conducted through definite 'wires' to definite spots in the brain, and that the properties of the local processes thus excited would have no influence on each other.

In passing we may draw attention to the fact that such an extreme 'machine theory' has never been seriously held in modern experimental psychology, at any rate since the time of Ewald Hering. It used to be considered that to every stimulus there corresponded a certain response, just as every time a key is struck on the piano a definite string vibrates. Since it was very soon found (e.g. by Helmholtz in his work on vision) that this was hardly ever the case, it was assumed that at any rate genetically this state of affairs must have been the primary one and that only in later stages of evolution, meaning and accumulated experience added something to these 'primary sensations.' But Hering and his school insisted on the 'autonomy of organic processes.' They drew attention to the fact that to one stimulus there could correspond whole ranges of sensational contents and that therefore psychological observations should start, not from an analysis of *physical* manifestations of matter (wave-lengths of light), but from sense experience itself. Katz, Jaensch and many others investigated the way in which the sensations of light depended on their spatial context. In connection with the phenomena of the 'constancy of brightness,' the approximate 'constancy of colour' and in the field of space perception, Jaensch and his pupils in particular were able to show that there is no such thing as a 'primary sensation.' On the contrary, the primary fact is the perception of 'things.' Helmholtz, in the chapters on spatial perception of his *Physiological Optics*, also characterized the perception of objects (i.e. 'wholes') as the factor which predominantly determines perceptual processes. Indeed, if this were not so, we could not understand how any object could be an 'invariant' for us. A piece of paper in the dim light of evening would look like coal—since photometrically coal in sunlight emits as much light per unit area as paper in a deep shadow—and every fleeting shadow or change in illumination would change the paper. That in the case of perceptions wholes are more than their parts, that, indeed, the parts are often determined by their whole context, is of fundamental biological significance. But the way in which they so depend cannot be explained from purely physical principles. "If dynamics are excluded from the determination of distribution, local process will correspond in every case to its stimulus; the actual properties of stimulation in their mutual relationships will play no rôle in the whole affair, as they would undoubtedly in the case of dynamical interaction[2]." This conclusion is drawn by Köhler first, because he has this curious 'static' idea of machine conduction, secondly because he thinks that order can only be achieved in the case of the type of 'whole' with which sensory experience deals, if there is 'dynamical interaction.'

[1] P. 120. [2] P. 127.

Köhler gives numerous examples of cases in which observation has proved that properties of local sensory stimulation cannot be responsible for the actual perceived facts or their internal order, such as seen movement (the stroboscopic experiments of Wertheimer and others), colour contrast, the Müller-Lyer illusions, tonal fusion, etc. The machine theory—"a policy of least scientific effort"—is unable to cope with them; the only solution is 'dynamical interaction,' which can, for instance, take place in the gray ganglionic and nuclear fields of the brain. Is this any vital advance? We are still dealing with purely physical mechanisms. Or does Köhler want to make us believe that dynamical distribution is not a physical mechanism? Indeed, no: for he quotes "a few out of the millions of examples" of such interaction and distribution—drops of water, charges, ions, etc., in support of his contention that physical systems also have *Gestalt* properties.

That is one of the characteristics of this book. A certain number of phrases are repeated over and over again, with tremendous emphasis, wherever more detailed explanation is called for. The result in the reader is eventually a conditioned reaction or a hypnotic trance: if the explanation of every phenomenon is "the dynamical distribution of process-in-extension," one eventually begins to believe it; sheer reiteration batters one into acquiescence. But what we are to make of many of the statements is not quite clear; a drop of water in a pipe can only move in one direction under pressure. But a drop in a large volume moves at each point along the resultant vector of forces, and 'dynamical distribution' takes place. "In pipes, order is produced by *exclusion* of dynamical interaction; whatever distribution may result in the 'dynamical case' is *produced* by dynamics itself[1]." What is the difference in the two cases? In the pipe, the pipe itself prevents the water from spreading out, but each molecule or drop moves along the resultant vector of impact forces from other molecules just as much as it does in the case of a drop in a basin, or ocean of water, where the surface of the large mass prevents the drop or molecule from leaving. In both cases there is exactly the same amount of dynamics. Each movement of any drop of water is rigidly determined, theoretically calculable and predictable in either case. All Köhler's examples are no more than tedious illustrations of the oldest law in physics: that of action and reaction. However, events of this (dynamical) type "are excluded almost completely from machines, and the same type of process is practically excluded by standard neurological and psychological theory. *Gestalt psychology asks*

[1] P. 135. Köhler's italics.

to know the ground for its exclusion and defiantly proposes to give this type of process a fundamental rôle in psychological theory[1]."

At this point Köhler states categorically that both to Aristotelians and to modern theorists dynamical interaction is a synonym for disorder, since *anything* may happen! It is beyond us to imagine any physicist making such a statement. Nor did Aristotle make it. To him there were, *in addition to* the mutual interaction of parts in any physical process, special 'tendencies' that brought about any particular state. This was also the physics of pre-Galilean times. Since Galileo the conception of a special 'tendency' for each event has been discarded, for obvious reasons. But Köhler introduces it again as 'new,' as we shall see.

Having now settled once and for all that the 'processes in the field' are dynamic, we must explain why the figures of the tablecloth and carpet before me remain separate, why they do not fuse into one vast dynamical organization, to give an orderly distribution of all the parts. We sometimes do get fusion, it is true, as in diffuse contours, or after the tenth whiskey. But on the whole perceived objects are clearly separated. How is that? The explanation, according to Köhler, is very simple. Take the case of oil in water. The interaction of molecular forces is so powerful that the surfaces become separated; in the case of alcohol and water they fuse. "I shall assume then, that, in optical processes, contours are preserved by similar *forces of antagonistic contact*, depending upon differences in the properties on the two sides of the contour." "Generally, processes corresponding to a definitely colored area will have definite properties as a class of processes, different from the properties of a surrounding class of processes which correspond to another color. They will remain isolated in the nervous network if we suppose that in the ganglionic fields, where they 'touch' each other, their differential properties act as *separating forces of contact*, so that they mutually exclude each other[2]."

We obviously cannot assume that, if I see a hundred different patterns in the carpet with thousands of different separating lines, there are just so many directions of these 'separating forces of contact.' We must assume either that there are so many thousands of forces; or that the few that are operative are static and malleable like wax; or that there would be a tendency to see in circles. According to Köhler, the last is actually the case. When an after-image dies down, it takes on a symmetrical form, and memory images of complicated figures get 'smoothed down' in the process of time. But what about my memory images of playing tennis? Later on we find more *ad hoc* hypotheses of the same type: boundaries are places "where forces of 'segregation' and 'separation' take the place of those of 'coherence' operating elsewhere[3]."

There is one further point of importance. In all these 'dynamical distributions' among drops of water, etc., we know that the acting forces

[1] P. 135. Author's italics. [2] P. 143. [3] P. 148.

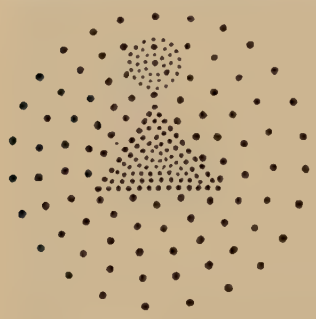
are constant. If they are not, *e.g.* if the water is stirred irregularly, there will be no dynamical equilibrium. That is why Köhler emphasizes that order is only produced in *undisturbed* dynamical interaction. But in organic systems we would seem to have precisely this irregular production of forces. Therefore we must either assume that 'dynamical processes-in-the-field' are not affected by them—and we obviously cannot make that assumption; or we may apply the principle of 'separating forces of contact'; or we can ignore this particular point and say "in organic systems all inertial velocities will be destroyed by friction," so that these systems will necessarily provide us with perfect examples of the distribution by dynamics leading to rest or stationary processes[1].

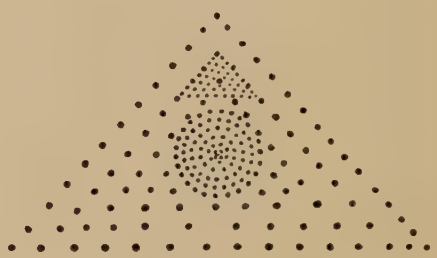
Köhler insists that segregation of wholes in the sensory field is a primary fact. Such wholes only afterwards acquire meaning. Indeed, *Gestalt* psychology "even goes so far as to hold that it is precisely the original organization and segregation of circumscribed wholes which make it possible for the sensory world to appear so utterly imbued with meaning to the adult, because, in its gradual entrance into the sensory field, meaning follows the lines drawn by natural organization[2]." Thus a person walking along in a mist may see certain segregated wholes without in the least knowing what they are. They may remain mere 'patches' until they begin to mean something, when their whole organization may become completely altered. But we must not attach any real importance to the influence of meaning. It is argued, for instance, that all the groups we know consist of physical objects, which are perceived as moving together. Therefore, whenever a group is seen, this is held to be some physical object, *i.e.* it has a meaning attached to it at once. But this is quite fallacious; for five flies sitting on a wall may be experienced as a perfectly definite group, although next minute they all begin to crawl in different directions. Again we ask, has this example really anything to do with the former argument? That concerned itself with more or less homogeneous groups, not with singular points. If one saw a continuous patch of the same size and boundary configuration as the group of flies, one would most certainly be surprised to find it moving in all directions, and would immediately take it to *mean* that it was a jelly, or a mass of minute black animals.

This again illustrates the difficulties behind the theory of 'separating forces of contact.' Consider the two figures on p. 84: one certainly experiences two 'groups' of dots. Therefore the underlying dynamical context is composed of two units or wholes. But each group has within it a further

[1] P. 139. [2] P. 152.

group, as well as a large number of isolated points. How does each point become differentiated within the dynamical unit by these separating forces? And why should we have separating forces here, but not if the interior of the two figures is entirely blacked out by microscopic points, whose distance apart is just too small to be separated by unaided vision? It is obvious that at any rate "the relations of neighbourhood, of similarity and difference among the stimuli, though these are indifferent to each other dynamically, are in some respects a copy of the corresponding relations among the surface-elements of surrounding objects. Some definite relations among the stimuli determining sensory organization, and the preservation of these relations in transmission, seem to be the essential conditions for that reconstruction of objective 'belonging





together'[1]." "The equality or similarity [of particles in a group] and their common difference from other 'particles' favor their becoming grouped together and their segregation from others[2]." The organization of the field is not independent of the *relative* properties of stimuli. But it is dependent on relations that are inevitable in a physical system. Just as groups of dots may be 'organized,' so taps may be 'organized' into groups of temporal 'dots.' "*These groups represent an example of physiological, or, if one likes, psychological organization*[3]."

Although the formation of groups, while being an elementary fact, nevertheless depends on certain relations of similarity and difference, the relations of these groups among each other, as well as any other psychological experience, depend on 'tendencies,' or on 'stress.' Thus "a change of attitude always involves a definite physiological stress exerted upon a sensory field by processes originating in other parts of the nervous system, and to some degree the organization of the field may yield to it[4]." Why is our attitude of dislike taken up towards some objects and

[1] P. 182. [2] P. 155.
[3] P. 164. [4] P. 184.

situations? Why do I move away from a radiator that is too hot? Because "there is a field of force tending to increase the distance between the place of the radiator and the place of the self[1]." When I begin to move away, that movement is experienced, and gives rise to a new physiological dynamical distribution, which is felt as being in the direction of the former stress. "Therefore, physiologically as well as in experience, what happens will be no more than the natural consequence, the real development, or *the evolution of something which, in germ, was already implicitly contained in the tendency*[2]." And this is no more and no less than *insight*[1]. In exactly the same way we must interpret instinct and reflex actions. The organism moves in the direction of the physiological stresses set up between various processes underlying various relevant experiences, and the instinctive action is no more than the outcome of the tendency of these stresses to come to a state of equilibrated dynamical distribution.

It is unnecessary to pile up further examples of the uses to which the fundamental 'radical physiological hypothesis' is put. Every 'direct experience' is the result of dynamical processes in the brain-field; action, insight, association, reproduction, observable behaviour, are all the outcome of dynamical tendencies in these processes themselves, or in their traces, which are probably some form of molecular concentration. We may admit that we can see no adequate reason why behaviour and reproduction should not be based upon something more complicated than a mere impress of disparate stimuli having no sort of relation to each other (except the inevitable ones of neighbourhood, similarity, etc.). We may agree with McCurdy that it has something to do with 'patterns,' or with Köhler that it is bound up with *Gestalten*. We shall also be inclined to agree with McCurdy that, whatever we may say about the probable organization of such patterns, the way in which they actually bring about, say, reflex activity, and their nature still remain "the fundamental mystery[3]." But Köhler will have none of this. If McCurdy patterns are 'a fundamental mystery,' it is quite obvious that he knows nothing about 'physical *Gestalten*[4]' as these have been elucidated by Köhler. What, then, are those properties of physical *Gestalten* that are the same as mental structures, or that are the root and final explanation of all experience?

[1] P. 390.

[2] P. 391. Author's italics. Insight is thus thought to be no more than a mechanical reflex.

[3] John T. McCurdy, *Common Principles in Physiology and Psychology*.

[4] W. Köhler, in his review of McCurdy's book in *Die Naturwissenschaften*, Heft 20, 1929.

These properties are very simple; so simple that it is extraordinary why physicists have not tumbled to them before. It would have saved psychologists a lot of trouble, for they need not have tried to base psychology on the mere senseless interaction of completely unrelated elements. "The dawn of the *Gestalt* problem in modern psychology was not the idea of dynamical self-distribution as opposed to order enforced by arrangement;...the starting-point was the observation that sensory fields are replete with qualities and properties which one neglects if one takes 'sensations' as their sole content and which, indeed, may have a mysterious aspect when first viewed in this way. It was von Ehrenfels who...directed the attention of psychologists toward the fact that a great many, and perhaps the most important, properties of sensory fields do not fit into the scheme of concepts which is centred around the idea of 'sensation'[1]."

The two properties which sensory wholes possess are best exemplified by a melody. If the different notes that make up a melody are presented separately to different people, the sum of their experiences will not be the same as the experience of one person to whom the melody is presented as a whole. To him it has something more than the sum of its parts, its specific melodic form. Furthermore, the melody will still be recognized if it is transposed to another key, that is, if every vibration is altered in a certain way. Chickens can be trained to respond to a certain shade of gray, which is the darker of two shades presented. If both these grays are changed, the chicken will respond to the gray which is the darker in this second setting. Their response does not depend on the absolute intensities of the stimuli at all; it depends on the whole situation and the relative properties of the stimuli. These, then, are the two fundamental properties of *Gestalten*: (*a*) the whole is more than the sum of its parts; (*b*) *Gestalten* (or wholes) can be transposed.

Now the fundamental psychophysical postulates (cf. p. 76) state that all experience is based immediately on physical processes, which are dynamical units. But a physiological process is an event in the material universe. Hence, "in order to prove that dynamical self-distribution will explain 'transposition,' we have to show that 'transposition' is possible in self-balanced physical systems. Nothing can be easier![2]"

Consider an electrolytic system containing Na and Cl ions. The charges will be carried equally well, and the 'self-distribution of the current' will remain the same, if we substitute Br and K ions. Again, the potential difference between two solutions is unaltered if the concen-

[1] P. 187. [2] P. 218.

trations of each are altered in the same proportion. The solution that was the electropositive side when the concentrations were $\frac{1}{20}n : \frac{1}{4}n$ will still be the electropositive side when the concentrations are $\frac{1}{100}n : \frac{1}{20}n$. "To be 'the electropositive side' of such a physical system is no less a *Gestalt property* in a definite electrochemical whole than to be 'the dark side' is a *Gestalt property* in a sensory pair[1]." In physical systems, therefore, we find precisely the same Gestalt properties as we find in a melody: if two solutions are kept separate, there will be no potential difference, nor has either solution alone a potential—*i.e.* the whole is more than the sum of its parts. Also the system can be transposed without altering this very characteristic, which appears only when the system is considered as a dynamic whole.

How is it, we may ask, that physicists have not discovered this before, so that it needed a psychologist to point out the existence of *physical Gestalten*? The answer is, first, that both von Ehrenfels' criteria mean one and the same thing; secondly, that physicists do, in fact, study nothing else, except that they call the physical *Gestalten* physical *systems*. This term has been in use for so long that there is no sense in changing it, less sense still in mixing it up with Aristotelian, pre-Galilean 'tendencies' or mystical conceptions about the potency of an '*eidos*.'

Köhler considers the first criterion necessary but not sufficient, the second sufficient but not necessary. Hence both must be applied together. Actually, however, they are synonymous, and not at all adequate. There can be no two or more things without there being relations between them. They will have relative geometrical positions. Since they form a manifold, the laws of mathematics, in particular the theory of groups, will apply to them. Thus they will belong, say, to the group of countable natural numbers and will share its properties *over and above* the mere fact of their arithmetical sum. That gives us our first criterion. But as soon as that condition is fulfilled, the group is transposable, for the theory of numbers does not deal with stones or cows, but with 'indefinables.' Similarly, an ellipse is more than a mere sum of geometrical points. It is also transposable into other co-ordinate systems, where its fundamental properties remain the same. To take a concrete example: any group of towns in England stand in a certain relation to each other, which is more than the sum of the parts. For, if Edinburgh were transported to Mars, it would no longer be 'north of' London, although the mere sum of the two towns has remained the same. But the relations between the

[1] P. 219.

towns remain the same when transposed to any number of maps of different scales. It is because relations do not depend on absolute values that both von Ehrenfels' criteria mean the same thing. And it is spatial and temporal relations that are measured by physics, before it begins to study causal relations. Indeed, the possibility of formulating a general law depends on the fact of 'transposableness.' The gas laws apply to O_2, Cl_2, N_2, and any other gases. Köhler cannot show us any physical system in which these criteria do not hold. But they are purely descriptive and tell us nothing more than: if there are parts, there will be something more as well, which vanishes when the parts are separated, and which remains when their relationships are altered proportionally. They are, in fact, simply another way of expressing the law of proportion; but they do not tell us whether it is the whole that determines the 'more,' or the parts, or neither.

There must be another reason for expecting so much from physical *Gestalten*. It is to be found in Köhler's repeated contention that the Gestalt is *produced* by the dynamics itself; that a dynamical distribution, as of current in our electrolyte, is something fundamentally different to a distribution enforced by topographical arrangement.

Moreover, we have the "superstition which is thousands of years old" that dynamical processes when left to themselves only produce chaos. Physics, Köhler thinks, explains physical processes purely mechanically, as in the kinetic theory of gases, by the motion of small particles; the distribution is one of chance. But no one ever uses the word 'mechanical' in this sense, that natural processes are summative combinations of mutually independent parts. A machine is a mechanism: but the *function* of every part is determined by that of every other. A drop of oil comes to rest in water. Is there a process connected with the drop that goes on independently of the processes in the water? "Suppose that somewhere in a factory HNO_3 were produced out of its elements and that in another part of the factory the product of that chemical organization were used to dissolve silver—would you say that the silver reacts to Nitrogen, Hydrogen and Oxygen? You certainly would not, because what happens to the silver depends upon that chemical organization, and it cannot be understood as a reaction either to those elements separately, or to the sum of them[1]."

If we look into these physical systems a little more closely, what do we find? In the case of dilute electrolytic solutions we need to know only two things: the ionic mobility, that is, the velocity of the ions

[1] P. 180.

which is found experimentally, and the laws of the kinetic theory of gases. When we know these, the potential difference that arises when two solutions are in contact can be explained purely *mechanically*. It can be calculated from the known ionic mobilities and the gas laws for any number of such systems in contact.

Again, no physicist believes that he cannot understand the reaction of silver for HNO_3 from his knowledge of those elements. He has studied the structures of the four atoms concerned; he knows, or hopes to know soon, what the resultant structure and properties of the HNO_3 molecule must necessarily be; from this and the known structure of the silver atom, the reaction follows necessarily, again on the basis of 'mechanical' laws. There is no 'tendency' in the HNO_3 and Ag to attract each other. It all follows from the laws of electricity. In the last resort, of course, these are based on the peculiar properties of positive and negative charges; and here we may speak of 'tendencies,' since we cannot as yet fathom all the depths of the universe. All of physics is perhaps built up on this *one* 'tendency,' not on millions of *ad hoc* 'tendencies.'

But this is not at all what we understand by 'tendency' in organic systems. What we mean when we say a process is 'mechanical' is that it is determined completely at every instant by x factors (charge, concentration, temperature, etc.), which do not *of themselves* belong together, through some inner necessity. These factors coming together, the law of action and reaction determines the entire process. If another factor is added, the process may turn out quite differently, while still remaining strictly predictable. That, indeed, is how the scientist works. He is always seeking out those x factors. When a process with which he is familiar suddenly takes another course, he does not imagine that it has got bored with the old ways and wants a change; he infers that a new factor has entered, which is contributing its quota of action and reaction, and sets himself to discover it. The relative causal interdependence of factors is therefore the fundamental presupposition of natural 'mechanism.' A chemical process is called 'mechanical' because none of the x factors belong together through inner necessity; nevertheless, once they are together, a variation in any one factor necessarily changes the process as a whole. We do not assume that two solutions come together because they want to produce a potential difference, which neither can produce by itself. If they happen to come together (through the intervention of the chemist), this potential difference is produced inevitably by mechanical laws, whose operation is fully determined and foreseeable at every instant. There is no "classification of

physical systems which is decisive for our problem[1]," into those with unrestricted dynamical possibilities and those with 'topographical' restrictions. Just as steam is prevented from leaving the cylinder by the walls, so are ions prevented from leaving solutions, or charges from the surfaces of conductors. All are physical systems. In each the distribution of process at any moment is determined by the number of reacting factors and forces.

It does not seem to us that the essential characteristics of the higher mental processes are in any way describable in the same terms. One of those characteristics is a true 'tendency' towards achieving some result and setting circumstances so that this result is achieved, however many external causal factors may try to prevent it. If I want to go from Cambridge to London, and start off by air, the aeroplane will get into a complicated set of cross-currents. Its tendency will be to move in the direction of the resultant vector of forces, which may point to Bath. But I control the aeroplane, and continually adjust its path. Then the engine fails, and I land in a field—but I shall get to London eventually. In *spite* of numerous deflecting causal factors, the object is achieved. In what way is this process comparable to the redistribution of charges that takes place when a certain amount of charge is taken from one plate of a condenser?

How do I see a circle according to Köhler? In the brain there will be two processes, corresponding more or less to the circle and to its surroundings. Between these two there is a dynamical mediator, *e.g.* there may be two molecular concentrations in the brain-field, between which a potential difference is set up. Why do I still see a circle when the illumination is changed, and the circle painted green? Because, although we now have two different concentrations, the potential difference has remained the same. It is quite inconceivable to Köhler how a circle could be given to me in direct experience if I only assume local, indifferent, machine-conducted processes. But it is immediately obvious why a potential difference can and *must* give me the direct experience of a circle! We fail to see why the one should be so much more obvious than the other.

There is another ground on which the assumption, that our experiences are based on purely physico-chemical *Gestalten*, can be shown to be false. This has been fully worked out by Jaensch[2]. It is the fallacy of substituting causal for ontological analogies. The argument may briefly be put thus: widely different spheres can be shown to have a large

[1] P. 112. [2] *Op. cit.*

number of corresponding properties. We have just seen that every physical system must necessarily conform to the two Ehrenfels criteria. But these are merely the expression of mathematical properties such as proportionality, which are shared by extremely diverse manifestations of the material universe. Certain fundamental mathematical functions recur, for instance, in the kinetic theory of gases, the theory of damped vibrations, the rate of growth of trees, the theory of economics, and the Weber-Fechner laws. Wherever the rate of increase of any system is proportional to the state of that system at each moment, some function of e (the base of natural logarithms) will occur, since the exponential function has the property that, if $y = e^x$, $\frac{dy}{dx} = e^x = y$, *i.e.* the increase dy of the function y is proportional to the original value. Would this fact, which to a superficial observer might appear to be of the most fundamental, if mysterious significance, lead us to suppose for one moment that there is any connection between the growth of trees and damped vibrations? No; the occurrence of such simple relations is a fact, not a necessity of thought, and it is the reason why mathematics can be the foundation for wide fields of knowledge, and why its conclusions are valid over a wide range. To argue that the different fields quoted above are connected, would be to argue on a false, *ontological* analogy.

A true, *causal* analogy is of a different kind altogether. To study the flow of a liquid composed of oil and water past symmetrical sections (aerofoils) because that can be seen and photographed, and then to apply the knowledge thus gained to the flow of *air* past similar aerofoils attached to aeroplanes, is to argue from a true, causal analogy, since the causal factors that are operative are the same in kind. No physicist who observes von Ehrenfels' criteria in two systems will come to the conclusion that they must necessarily have anything in common beyond these relationships, which apply indiscriminately to *every* group of objects. But that is what *Gestalt* theory does. *For the ontological analogies that are expressed in von Ehrenfels' criteria are taken to be a proof that physico-physiological and psychological* Gestalten *have a necessary inner relationship, and that the structure of mental processes is that of these—purely hypothetical—physiological processes.*

We mentioned before how much use Köhler makes of 'tendencies.' A thirsty man experiences a feeling of pleasure when drinking a glass of beer on a hot day. The layman feels at once, Köhler says, that the pleasure is associated with the beer and the coolness of it, and not with the buzzing flies, the orchestra, a smell of garlic, the bustling waiters or

anything else; it is the direct outcome of cool beer and parched throat. Now let A = the process underlying the direct experience of coolness and taste, B = the process underlying experienced thirst. Both processes take place "in that part of the brain-field which corresponds to the self." The pleasure is the direct outcome of an immediate influence exerted by process A on process B. The processes corresponding to the experienced sounds from the orchestra or clattering plates happen not to have any tendencies to move in the direction of A or B[1]! Whatever happens in experience, it is the outcome of some tendency, just as, *e.g.*, condensers have the 'tendency' to re-establish a uniform distribution of charges after some charge has been withdrawn from one plate. That the two types of 'tendency' have nothing to do with one another is immediately obvious if we ask the following question: Would the re-establishment of a uniform distribution take place *in spite of* opposed causal factors? If it would, then if one of the plates of the condenser were bent, it should *of itself* bend straight again, since a uniform distribution is only possible over parallel plates. This does not happen, because the distribution has nothing whatever to do with any tendency; the form of the plates is the only condition, whereas the distribution is an invariant towards such external factors as the taking away of a definite amount of charge.

Throughout the book Köhler is at great pains to dissociate himself entirely from the older materialism in psychology. But it does not seem to us that he has succeeded either in escaping from the older views by enunciating radically new psychological principles, or in applying other than purely physical principles, which have nothing to do with psychology as such. Let us take the latter contention first.

The formal structure of all experience—and in this we must include association—is the same as that of the dynamic units on which it is based. The organization of these units is *produced* by dynamics, because it is due to the relative properties of the interacting physiological processes. But we have seen that this 'radical physiological hypothesis' is based on a false *ontological* analogy. The specific *Gestalt* properties enunciated by Köhler are common to the whole material—and mathematical—universe[2]. But on this view organization must be purely fortuitous, and we are back at the biological mechanics of Weissmann, which Bernard Shaw caricatures so brilliantly in one of his prefaces.[3]

[1] Cf. chapter on "Insight," in particular, pp. 375–376; 389–390.

[2] Even in the new non-commutative Algebra of Darwin and Dirac von Ehrenfels' properties hold.

[3] *Back to Methuselah.*

Meaning, according to Köhler, follows the lines laid down by natural organization. It is curious that this should generally be of biological significance, while the organization of physical systems is not. "Where organization is naturally strong we have spontaneous association; where there is practically no organization association does not occur until some organization is created intentionally[1]." The first part of this sentence admirably describes the fortuitousness of the process. *But the second part touches the vital psychological point.* In physical systems there is never any such *intentional* organization. It is characteristic of the mind and no amount of physico-physiological theorizing will locate this intention, a real psychological 'tendency,' in the dynamic 'structure' of physiological brain-processes.

Let us return to our first contention, that nothing radically new has been proposed. The principle of 'naïve description' (p. 76) always has been and must necessarily be a primary postulate for all psychological work except, perhaps, in work on conditioned reflexes or purely physiological and chemical events. Even in experiments on physiological after-images we must believe what the subject tells us.

The 'first primary psychological postulate' (p. 77) seems to be introspection in a slightly different form. For does not introspection use 'direct experience' "in order to guess at processes about which we wish to formulate a working hypothesis"? We may admit that introspection very often changes the field, and that introspectionists nevertheless believe these changed phenomena to be the true ones. But the point of view, or belief, remains the same.

The 'psycho-physical theorem,' on the other hand, argues in a circle on the basis of a non-existing intermediary link. We do not construct the physical world merely by uncaused endogenous acts of thought. We isolate certain aspects of reality, control the various operative factors in turn, and observe the results. This method gives constructive thought the cud for philosophical rumination, which finally produces the milk: our picture of the physical world. The intermediary here is our direct experience (of scales, etc.). But we cannot construct the "physiological processes directly underlying experience" in the same way, since there is no further intermediary. We could only construct them as part of *physical* reality by isolating them as such; and the work of Adrian on the eel's retina, of Lashley on cerebral localization in rats, and of many other physiologists has shown that the physiological processes corresponding to some definite 'direct experience' cannot be

[1] P. 290.

isolated. Any one stimulus can give rise to completely different ranges of experience, because of 'the autonomy of organic processes.'

About the other axioms and inferences it is needless to say more. They are hypotheses and as such have been well presented by Köhler. In his preface he apologizes for his inadequate presentation owing to linguistic difficulties. We can only congratulate him on his success, and wish that many English and American psychologists could write as clearly and incisively. The difficulty of the book is not the language, but the hypotheses. It is good to insist, as Köhler does, that until really adequate quantitative methods are found, psychology should be content with qualitative observation and should not be bound by inflexible theories taken from physiology or physics. But in this book there is no detail whatsoever of apparatus or controls; qualitative observations are few and chiefly prove that the perception of wholes is a primary fact—which is widely realized in present-day psychology.

OBITUARY.

PROFESSOR MARY WHITON CALKINS.

THE news of the death of Prof. Mary Whiton Calkins on February 26th was received in this country with great regret.

For more than forty years Prof. Calkins had been associated with Wellesley College, Mass. From 1898 to 1929 she held the chair of philosophy and psychology, and in 1929 she was elected to a research professorship. She was a pupil of Prof. William James and of Prof. Münsterberg, and was greatly influenced by the philosophic teaching of Prof. Royce. She has stood as the exponent of personalistic or self-psychology and the value of her work has won widespread recognition. She received the honorary degree of Litt.D. from Columbia University in 1909 and that of LL.D. from Smith College in 1910. Her last visit to England was in December 1927, when on the invitation of the University of London she gave a short course of lectures. Consequent on this visit she was elected an Honorary Member of the British Psychological Society, and is the only woman to have received this honour.

Prof. Calkins made many important contributions to philosophical and psychological journals. An article by her, "The Limits of Genetic and Comparative Psychology," appeared in the first volume of this *Journal*. She contributed an article, "McDougall's Treatment of Experiment," to vol. XIII, and a very characteristic article, "Converging Lines in Contemporary Psychology," to vol. XVI. Her two psychological text-books, *An Introduction of Psychology* and *A First Book in Psychology*, and her book *Persistent Problems in Philosophy* have been widely read by students in this country. As a teacher Prof. Calkins not only had the gift of clear exposition but also the power of arousing in others the enthusiasm she herself felt for her subject. She was an intelligent critic, scrupulously fair, and keenly alive to difference in standpoint. She took an interest in the social and political problems of the present day and applied to practical life the principles underlying her philosophy. In this she was a worthy representative of a family directly descended from the *Mayflower* pilgrims. Miss Calkins' personal qualities —not the least of which was a sense of humour—endeared her to all who knew her. She will be greatly missed by a wide circle of friends.

B. E.

PUBLICATIONS RECENTLY RECEIVED

Principles of Experimental Psychology. By HENRI PIÉRON. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1929. Pp. viii + 186. 18*s.* 6*d.*

This is an exceedingly valuable book which ought to be readily accessible to every student of psychology. Although it contains a great amount of well-selected and well-attested information, it is not a text-book in the ordinary sense of the word. It will however powerfully help the student in his most difficult task, that of attaining an orientation towards the mass of often conflicting data and opinions that go to make up contemporary psychology. Prof. Piéron has set about his task in no narrow spirit, and he includes in his study historical notes and no small amount of information about human reactions in general, emotional and affective as well as cognitive, social as well as individual and clinical as well as experimental in the narrower sense. On every one of his topics he writes lucidly and concisely, and he is throughout interesting to a high degree. In short the volume deserves a really warm welcome on the part of all those who take psychology seriously. The translation by Dr J. B. Miner is excellent.

The Common Sense of Dreams. By H. G. WATT. Worcester, Massachusetts: Clark University Press (London: Humphrey Milford). 1929. Pp. xvi + 212. 13*s.* 6*d.* net.

In one respect at least this book serves to emphasize the great loss to English psychology which was suffered by the early death of its author. *The Common Sense of Dreams* is incomparably more direct and clear than any other published work of Dr Watt. At the same time its originality is less marked and is on the whole of a rather derivative kind. The book begins by a detailed study of some of Rivers' published dreams and proceeds in an atmosphere of concrete analysis. Watt, like most of his predecessors in this field, seemed to want a formula for the psychological explanation of all dreams. He was however dissatisfied with the notion of wish fulfilment and also with that of 'conflict' mainly, apparently, because of the part played in them by hidden and inscrutable factors. His own formula is that the dream is "the solution of some reluctance or reluctances." That is to say a train of thought is started in waking life which is for some reason held up. The dream provides a setting (also coming from waking experience) through which the 'held up' thought can work. With this general principle in mind Watt gives examples of dream interpretation, discusses reflection in the dream, attacks nightmares, and provides a good deal of forceful criticism of other theories. The book is well named. Watt will have common sense—guided naturally by a lively and accurate knowledge of psychology—and nothing but common sense throughout. It is well worth reading, and in particular many of his criticisms are extremely well directed, while they are always both serious and good humoured. Dr Shepherd Dawson contributes a brief sketch of Watt's career, and a bibliography of his publications.

Experiments in General Psychology. By NORMA V. SCHEIDEMANN. Chicago: University of Chicago Press. 1929. Pp. 108. 4*s.* 6*d.* net.

This simple 'note-book manual' for the student describes a considerable number of experiments of a psychological character all of which can be carried out without elaborate apparatus. It should be of considerable interest to anybody who wishes to know something in a general way about psychological experiments, and as an introduction to a more thorough treatment may serve a useful end.

Developing Appreciation through Teaching Literature. By Angela Marie Broening. Johns Hopkins Studies in Education, No. 13. Baltimore: Johns Hopkins Press. Pp. ix + 118. $2.00.

Dr Broening's problem was "How to teach literature so that boys and girls will love it and voluntarily read it for the satisfaction of the literary experiences it affords." With great energy and obvious enjoyment she attacked this problem in an experimental manner, and her monograph gives a lively and well-illustrated account of the results. She claims to have found a way: the enthusiasm of the teacher plays a great part. The monograph is full of interest and should be of practical value as well.

Academic Progress: A Follow-up of the Freshmen entering the University in 1923. By Harold A. Edgerton and Hubert A. Toops. Ohio State University Press. 1929. Pp. x + 150.

This is a detailed statistical study of the progress through the University of Ohio of a group of students throughout a period of four years. It appears that persistence is not accurately predicted by any of the measures commonly used for predicting academic success. The best 10 per cent. of students (as selected by Intelligence Tests) give good results, and the worst 10 per cent. bad results relatively speaking, but apparently in the case under study the difference of academic attainment falls considerably below that of intellectual promise.

The Anatomy and Physiology of Capillaries. By August Krogh. Yale University Press. 1929. 18*s.*

This book contains lectures given in America in 1922 and now brought up to date (July 1928, although published in 1929). Obviously the work is intended for histologists and physiologists, but no psychologist interested in skin sensibility can afford to remain in ignorance of it. Krogh shows how many are the reactions in the skin to stimuli which we interpret as different kinds of sensation. It is quite possible that punctate sensibility will turn out to be a matter of loci favourable for the eliciting of one kind of skin reaction rather than another and not to depend on the presence of specific nerve endings. This field of inquiry is new and agreement on all points of importance for the psychologist has not yet been reached. Krogh's work should, therefore, not be accepted without reading as well the book by Sir Thomas Lewis, *The Blood Vessels of the Human Skin and Their Responses*, London, 1927.

A Text-Book of Psychiatry for Students and Practitioners. By D. K. Henderson and R. D. Gillespie. Second Edition. Oxford University Press. 1930. Pp. x + 526. 18*s.* net.

The early appearance of a second edition of this work is very welcome. It is in fact far the best general text-book in psychiatry that has been published in this country, whether for practical or theoretical purposes. The authors are thoroughly alive to the psychological side of their topic and are as clear, precise and informative as it would be possible to be within reasonable limits of exposition. Nobody who has had to do with the training of medical students on the functional side can fail to have noticed the growing interest in and capacity to tackle the mental problems of medical science which have been manifested during the last few years. For the further development of this interest and capacity no better book than this—so far as its specific topics go—could be desired. The second edition has not been changed very much, but has been brought up to date by a few necessary additions.

The Neuroses. By I. S. Wechsler. London: H. B. Saunders Co. 1929. Pp. 330. 18*s.*

This is a clearly written and most useful general manual. It contains a brief historical introduction followed by chapters dealing with the 'mental mechanisms' that are of special importance for the psychopathologist, with 'the etiology of the neuroses';

with 'classification of the neuroses'; with 'clinical manifestations of the neuroses'; with the 'diagnosis, course, and prognosis of the neuroses;' and with 'the treatment of the neuroses.' An appendix deals mainly with more immediately practical matters, concerning the history and examination of patients, general intelligence in relation to psychological disorders and the use of psychometric tests. A short Bibliography is added. The book cannot claim any very marked originality but it is written from a sane psychological point of view, the case material is sufficient and well chosen, and the exposition is straightforward. Taken all in all, it is a reliable and sensible book.

La Rève. Par MARTIN GOMES. Rio de Janeiro: Rodrigues and Co. 1928. Pp. 179.

This study of dreams considers their main stimulating causes to be various psychological tendencies which are separately discussed. A great many examples of dreams are given and considered, and the author shows a wide knowledge of contemporary literature. While nothing very new emerges from his study, he has collected a great amount of interesting data and interprets them in a sane manner according to approved psychological principles. The book is badly printed and contains numerous typographical errors.

Problems of Neurosis. By ALFRED ADLER. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1929. Pp. xxxvii + 178. 8*s.* 6*d.* net.

In this book Adler expounds his views simply and by the help of a number of case histories. He adds little or nothing to what he has said before, but writes with great clarity in a manner that will, beyond doubt, appeal to the general reader. In fact this is the best book that has appeared so far to recommend to the generally educated person who wishes to learn Adler's views. There is an appreciative and able preface by Dr F. G. Crookshank.

Psycho Analysis and Art. By GEORGE WHITEHEAD. London: John Bale, Sons and Danielsson, Ltd. 1930. Pp. 146. 5*s.* net.

Part I is concerned with anarchy and authority. It detects and deplores a growing anarchy in psychology, in sex relations, and in industry and politics. It insists that repression is called for in all these fields. Part II deals with sex and decorative art, with the wonders of instinct and with the wonders of intelligence. Nothing new emerges from the discussion. Part III deals scrappily and with much quotation with a diversity of topics: day dreams, racial phantasies, the evolution and functions of art. In general artistic gratification is for the purpose of sublimating "crude desires and to generate fervour for personal and social sacrifice." Part IV is called "The Psychology of the Penny Dreadful," but it deals with a large number of topics and does little more than repeat that what is chiefly needed in present-day society is the satisfactory sublimation of crude instincts. The whole book lacks rigour and organization and is disappointing.

The Art of Interrogation. By E. R. HAMILTON. (Introduction by C. SPEARMAN.) London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1929. Pp. xii + 171. 7*s.* 6*d.* net.

Most examiners and nearly all examinees at some time or another doubt the value of examinations. In this decidedly interesting book they may find many of the reasons for doubt clearly stated and also some reasons for belief as admirably put forward. The whole discussion is in fact interesting and attractive, and though it proceeds upon a rather general plane for the most part, is obviously the outcome of much thoughtful concrete exploration of its topic. The sub-title of the book is "Studies in the principles of mental tests and examinations." This accurately indicates its subject-matter, but hardly indicates how positive is the value of Mr Hamilton's contribution. Every cautious psychologist must now admit that the unthinking

multiplication of 'tests' for every conceivable reaction is one of the greatest dangers of his rapidly developing science. Mr Hamilton has thought well and worked well about this, and as a result he not only puts forward admirably pointed criticism but he also indicates how the 'test movement' may develop along sound lines. Prof. Spearman's appreciative introductory note is thoroughly justified by the book's performance.

Philosophie der Symbolischen Formen. Von Ernst Cassirer. Dritter Teil: Phänomenologie der Erkenntnis. Berlin: Bruno Cassirer. 1929. S. 572. M 22.

It is impossible here to do more than mention the appearance of this enormous volume, which should be very carefully studied by all those who are primarily interested in the epistemological implications of psychology. The book contains three main sections, dealing with: *Ausdrucksfunction und Ausdruckswelt*; *Das Problem der Repräsentation und der Aufbau der anschaulichen Welt*, and *Die Bedeutungsfunction und der Aufbau der wissenschaftlichen Erkenntnis*. The whole is an extremely able and learned treatise, though many readers will find it far from easy to make the necessary transitions from experimental and empirical data to far reaching epistemological formulations.

Foundations of Geometry and Induction. By Jean Nicod. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1930. Pp. 286. 16*s.* net.

Psychologists with a taste for epistemology and logic will enjoy this very able volume. Nicod attempts to begin with data of perception and from these to build up various geometries which can be said to be based upon them. Naturally the results often seem to have little relation to the concrete and complex facts of everyday life; but the argument is always stimulating and vigorous.

The Nature of Life. By Eugenio Rignano. London: Kegan Paul, Trench, Trubner and Co., Ltd. International Library of Psychology, etc. 1930. Pp. x + 168. 7*s.* 6*d.* net.

This is the most complete and concise exposition by Rignano of his views that has appeared in English. In it he applies his contention that the explanation of all living reactions must include a demonstration of determination by purposiveness to a wide range of phenomena, from those of elementary physiological functions to those of complex social orders. He also maintains that his view represents a half way house between vitalism and mechanism, having the excellencies of both, the drawbacks of neither, and other values all its own.

Die eidetische Anlage der Jugendlichen in ihrer Beziehung zur Künstlerischen Gestaltung. Von Dr Paul Metz. Langensalza: Herrmann Beyer und Söhne. 1929. S. 114. 2,70 R.-M.

The eidetic images of children are found to be on the whole extraordinarily plastic. In particular, the form is more easily changed than the colours in the case of 'basedow' types; the eidetic images of 'tetanoid' children are more rigid. But in both cases every attempt to copy them by running a pencil along them failed, because the images move in the direction in which the pencil moves, and are distorted in various ways. The author shows that the act of drawing of the child is based on dynamic, or motor patterns, and is a kind of language. The ideoplastic schema of children's drawings is developed at the same time as language, which is proved by the fact that the drawings of deaf and dumb children are physioplastic. Nevertheless, eidetic images have a most important function in providing the child with picture material, which can be moulded in various ways.

Die Psychologie des Grundschulkindes in ihrer Beziehung zur Kindlichen Gesamtentwicklung. Von Oswald Kroh. Langensalza: Herrmann Beyer und Söhne. 1930. S. 345, 6,60 R.-M. 4th–6th edition.

This book provides a clear exposition of the various phases in the development of the child, and analyses the development of ego-experience, perceptions, thinking, imagination, and the growth of ethical, religious, aesthetic and social experiences during the primary school age. An attempt is made to classify school children into the various types of Kretschmer. The author pleads for a far wider basis of education in the primary schools, because the plasticity and receptivity of the child at this age give it unlimited potentialities, if these are stimulated at the right time.

Über Gestaltpsychologie und Gestalttheorie. Von Prof. Dr E. R. Jaensch und Dr L. Grünhut. Langensalza: Herrmann Beyer und Söhne. 1929. S. 167. 5,40 R.-M.

The authors have given a lucid, critical analysis of the basis on which *Gestalt*-psychology rests. They show that it is a thoroughgoing materialism, veiled behind a misleading terminology and fundamentally false assumptions about the nature of the physical world. The physical *Gestalten* on which all experience and ideas are supposed to rest are shown to be merely physical systems, whose 'behaviour' is completely predictable from the nature of their parts; they are not teleological 'wholes' at all. Moreover, *Gestalt* psychology has misunderstood the meaning of the Kantian categories, and substituted ontological for causal analogies.

Empirisch-psychologische Beiträge zur Typologie des dichterischen Schaffens. Von Dr Berthold Leinweber. Langensalza: Herrmann Beyer und Söhne. 1929. S. 96. 2,40 R.-M.

A psychological analysis was made of the working methods of a group of writers by means of experimental tests and an examination of the type of symbolism each employs. On the results of these tests they are divided into three types: the dynamic, the naive or 'coherent,' and the contemplative. The results are also compared with the more general classification of the Marburg School into various 'integrate' types.

Die Lehrbarkeit der Religion. Von Dr Emil Schorsch. Langensalza: Herrmann Beyer und Söhne. 1929. S. 96. 2,10 R.-M.

The author defines the nature of religion as the reciprocal activity between the individual and objective Being. The child can be taught to experience this provided the teacher's religious life is in all respects an inner unity, and provided account is taken of three types of religious expression: intellectual, voluntaristic and emotional.

Wirklichkeit und Wert in der Philosophie und Kultur der Neuzeit. Von Erich Jaensch. Berlin: Otto Elsner. 1929. S. xvi + 251.

This work is intended as *prolegomena* to a reconstruction of philosophy on the basis of philosophical anthropology and *Wirklichkeitsphilosophie*. The advent of naturalistic positivism, and its attempt to explain everything, including consciousness, on purely mechanical principles, forced philosophy to retire more and more into 'unreal' spheres, to seek the peculiar functions of consciousness in some transcendental basic world underlying physical phenomena. But both positivism and idealism were bound to fail, since the one banished values and tendencies towards achieving and realizing such values from the universe because they are not to be found in inorganic nature, while the other sought them in unreal spheres. This phase of speculative endeavour is critically examined, and its limitations are set forth. Both are found to have their origin in Cartesianism, and this is shown to be limited by the experience and the mode of thinking of a certain psychological type. Since our modes of experience and of thinking are bound by our type, a reconstruction of the theory of categories becomes necessary.

Grundformen menschlichen Seins, mit Berücksichtigung ihrer Beziehungen zur Biologie und Medizin, zur Kulturphilosophie und Pädagogik. Von Prof. Dr Erich Jaensch (und Mitarbeitern). Berlin: Otto Elsner. 1930. S. 524, 42 illustr. 23 R.-M.

The long series of researches on eidetic imagery carried out in the Marburg Institute led Jaensch and his pupils into the psychology of types. It was found that certain fundamental modes of response were made by the same subject in every experiment and that the same general psychological structure was traceable from the lowest levels (physiological after-images, psycho-reflex of the pupil) to the highest (synaesthesias, the will, formation of concepts and values). In this volume a number of researches are collected, whose aim was the analysis of 'integrate' and 'disintegrate' forms of personality.

In the first part Jaensch shows the necessity for a thorough psychology of types and its application to philosophy, medicine and paedagogy. He discusses the experimental facts that have led him to formulate the concept of 'integration' and 'disintegration' as regards the various structural levels of consciousness, and shows that, from a biological point of view, the integrate type is a fundamental one. What is interesting about his methods is that it now becomes possible to combine rigorously experimental work with biographical and 'understanding' or 'interpretive' psychology, to give a complete picture of the whole structure of a personality. The study of such a personality as a type can then throw much light on questions of race psychology as is shown in the appendix, where Jaensch gives a psychological character-sketch of Stresemann.

The other parts of the volume are experimental researches by various collaborators, on pronounced cases of the *B*-type and integrate types generally; on the intermediary and transition types from integration towards disintegration; on synaesthesias and the type of personality correlated to them; and on the psycho-reflex of the pupil, a particularly accurate piece of work.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETINGS.

March 8, 1930. "Children's Thought." By Miss V. Hazlitt, D.Litt.

" " "Imagery and Learning." By Miss A. M. Jenkin, B.A.

April 26, 1930. "A Critical Examination of Köhler's Gestalt Psychology." By Dr O. A. Oeser.

" " "Psychological Factors in Peripheral Vision." By Mr G. C. Grindley.

" " "A Photographic Study of Eye Movements in Reading." By Miss M. D. Vernon.

SECTIONS.

MEDICAL.

January 29, 1930. "Diagnostic Significance of Sensory Aurae Epilepsy." By Dr John MacCurdy.

February 26, 1930. "On the Psychology of Values." By Dr M. D. Eder.

March 11, 1930. (*Joint Meeting with Psychiatry Section.*) A Symposium on "The Psychotherapy of the Psychoses." By Dr Devine and Dr Edward Glover.

April 30, 1930. "Some Points of Disagreement with Freudian Practice and Theory." By Dr Hilda M. Weber.

EDUCATION.

January 7, 1930. (*Joint Meeting with Nursery School Assoc.*) "Principles and Practice in Nursery School Education." By Mrs Susan Isaacs, M.A., and Miss Lillian de Lissa.

February 3, 1930. "An Investigation into the Relation between Intelligence and Inheritance." By Miss Evelyn Lawrence, Ph.D.

AESTHETICS.

March 13, 1930. Prof. Lascelles Abercrombie upon his Essay on a "Theory of Art."

INDUSTRIAL.

February 25, 1930. "Some American Approaches to the Study of Temperament." By Marion Milner, B.Sc.

JOINT MEETINGS.

EDUCATION AND INDUSTRIAL.

March 24, 1930. Papers on: "The Relation of Raising the School leaving Age to Problems of Vocational Guidance and Selection," contributed by Miss SHEILA BEVINGTON, B.Sc., from the point of view of Vocational Guidance,—and "Thomas Tibbey," from the point of view of Education.

EDUCATION AND AESTHETICS.

May 5, 1930. "The Child's Innate Sense of Music." By WILLIAM PLATT.